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ON LIGHT AND OTHER HIGH FREQUENCY PHENOMENA.*

BY NIKOLA TESLA.

[*Continued from p. 98.*]

When a conducting body is immersed in air, or similar insulating medium, consisting of, or containing, small freely movable particles capable of being electrified, and when the electrification of the body is made to undergo a very rapid change—which is equivalent to saying that the electrostatic force acting around the body is varying in intensity—the small particles are attracted and repelled, and their violent impacts against the body may cause a mechanical motion of the latter. Phenomena of this kind are note-

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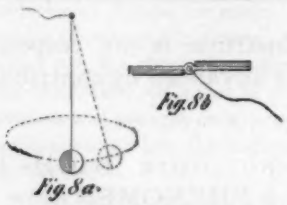
worthy inasmuch as they have not been observed before with apparatus such as has been commonly in use. If a very light conducting sphere be suspended on an exceedingly fine wire, and charged to a steady potential, however high, the sphere will remain at rest. Even if the potential be rapidly varying, provided that the small particles of matter, molecules or atoms, are evenly distributed, no motion of the sphere should result. But if one side of the conducting sphere is covered with a thick insulating layer, the impacts of the particles will cause the sphere to move about, generally in irregular curves, *Fig. 8a.* In like manner, as I have shown on a previous occasion, a fan of metal sheet, *Fig. 8b*, covered partially with insulating material as indicated, and placed upon the terminal of the coil so as to turn freely in it, is caused to spin around.



Mechanical motions produced by varying electrostatic force in a gaseous medium.

All these phenomena you have witnessed and others which will be shown later, are due to the presence of a medium like air, and would not occur in a continuous medium. The action of the air may be illustrated still better by the following experiment. I take a glass tube *t*, *Fig. 9*, of about an inch in diameter, which has a platinum wire *w* sealed in the lower end, and to which is attached a thin lamp filament *f*. I connect the wire with the terminal of the coil and set the coil to work. The platinum wire is now electrified positively and negatively in rapid succession, and the wire and air inside of the tube are rapidly heated by the impacts of the particles, which may be so violent as to render the filament incandescent. But if I pour oil in the tube, just as soon as the wire is covered with the oil, all action apparently ceases and there is no marked evidence

of heating. The reason of this is that the oil is a practically continuous medium. The displacements in such a continuous medium are, with these frequencies, to all appearance incomparably smaller than in air, hence the work performed in such a medium is insignificant. But oil would behave very differently with frequencies many times as great, for even though the displacements be small, if the frequency were much greater considerable work might be performed in the oil.

The electrostatic attractions and repulsions between bodies of measurable dimensions are, of all the manifestations of this force, the first so-called *electrical* phenomena noted. But though they have been known to us for many

Fig. 9

Showing the effect of the air.

centuries, the precise nature of the mechanism concerned in these actions is still unknown to us, and has not even been quite satisfactorily explained. What kind of mechanism must that be? We cannot help wondering when we observe two magnets attracting and repelling each other with a force of hundreds of pounds with apparently nothing between them. We have in our commercial dynamos magnets capable of sustaining in mid-air tons of weight. But what are even these forces acting between magnets when compared with the tremendous attractions and repulsions produced by electrostatic force, to which there is apparently no limit as to intensity. In lightning discharges, bodies are often charged to so high a potential that they

are thrown away with inconceivable force and torn asunder or shattered into fragments. Still even such effects cannot compare with the attractions and repulsions which exist between charged molecules or atoms, and which are sufficient to project them with speeds of many kilometres a second, so that under their violent impact bodies are rendered highly incandescent and are volatilized. It is of special interest for the thinker who inquires into the nature of these forces to note, that whereas the actions between individual molecules or atoms occur seemingly under any conditions, the attractions and repulsions of bodies of measurable dimensions imply a medium possessing insulating properties. So, if air, either by being rarefied or heated, is

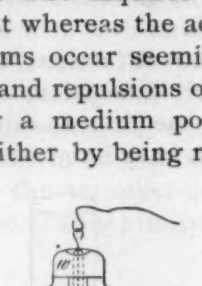


Fig. 10

Showing the influence of the conductivity of the medium upon electrostatic actions through measurable distance.

rendered more or less conducting, these actions between two electrified bodies practically cease, while the actions between the individual atoms continue to manifest themselves.

An experiment may serve as an illustration and as a means of bringing out other features of interest. Some time ago I showed that a lamp filament or wire mounted in a bulb and connected to one of the terminals of a high tension secondary coil is set spinning, the top of the filament generally describing a circle. This vibration was very energetic when the air in the bulb was at ordinary pressure, and became less energetic when the air in the bulb was strongly compressed. It ceased altogether when the air was exhausted so as to become a comparatively good con-

ductor. I found at that time that no vibration took place when the bulb was very highly exhausted. But I conjectured that the vibration which I ascribed to the electrostatic action between the walls of the bulb and the filament should take place also in a highly exhausted bulb. To test this under conditions which were more favorable, a bulb, like the one in *Fig. 10*, was constructed. It comprised a globe *b*, in the neck of which was sealed a platinum wire *w*, carrying a thin lamp filament *f*. In the lower part of the globe a tube *t*, was sealed so as to surround the filament. The exhaustion was carried as far as practicable with the apparatus employed.

This bulb verified my expectation, for the filament was set spinning when the current was turned on, and became incandescent. It also showed another interesting feature, bearing upon the preceding remarks; namely, when the filament had been kept incandescent some time the narrow tube and the space inside was brought to an elevated temperature, and as the gas in the tube then became conducting, the electrostatic attraction between the glass and the filament became very weak or ceased and the filament came to rest. When it came to rest it would glow far more intensely. This was probably due to its assuming the position in the centre of the tube where the molecular bombardment was most intense, and also partly to the fact that the individual impacts were more violent and that no part of the supplied energy was converted into mechanical movement. Since, in accordance with accepted views, in this experiment the incandescence must be attributed to the impacts of the particles, molecules or atoms, in the heated space, these particles must, therefore, in order to explain such action, be assumed to behave as independent carriers of electric charges immersed in an insulating medium; yet there is no attractive force between the glass tube and the filament because the space in the tube is, as a whole, conducting.

It is of some interest to observe, at this point, that whereas the attraction between two electrified bodies may cease owing to the impairing of the insulating power of the

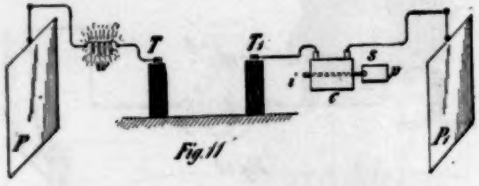
medium in which they are immersed, the repulsion between the bodies may still be observed. This may be explained in a plausible way. When the bodies are placed at some distance apart in a poorly conducting medium, such as slightly warmed or rarefied air, and are suddenly electrified, opposite electric charges being imparted to them, these charges are equalized more or less by leakage through the air. But if the bodies are similarly electrified, less opportunity is afforded for such dissipation, hence the repulsion observed in such case is greater than the attraction. Repulsive action in a gaseous medium is, however, as Professor Crookes has shown, enhanced by molecular bombardment.

On Current or Dynamic Electricity Phenomena.—So far, I have considered principally effects produced by a varying electrostatic force in an insulating medium, such as air. When such a force is acting upon a conducting body of measurable dimensions, it causes within the same, or on its surface, displacements of the electricity and gives rise to electric currents, and these produce another kind of phenomena, some of which I shall presently endeavor to illustrate. In presenting this second class of electrical effects, I will avail myself principally of such as are producible without any return circuit, hoping to interest you the more by presenting these phenomena in a more or less novel aspect.

It has been for a long time customary, owing to the limited experience with vibratory currents, to consider an electric current as something circulating in a closed conducting path. It was astonishing at first to realize that a current may flow through the conducting path even if the latter be interrupted, and it was still more surprising to learn, that sometimes it may even be easier to make a current flow under such conditions than through a closed path. But that old idea is gradually disappearing, even among practical men, and will soon be entirely forgotten.

If I connect, by means of a wire, an insulated metal plate *P*, *Fig. 11*, to one of the terminals *T*, of the induction coil though this plate be very well insulated, a current passes through the wire when the coil is set to work. First, I wish

to give you evidence that there is a current passing through the connecting wire. An obvious way of demonstrating this is to insert between the terminal of the coil and the insulated plate a very thin platinum or German-silver wire *w*, and bring the latter to incandescence, or fusion, by the current. This requires a rather large plate or else current impulses of very high potential and frequency. Another way is to take a coil *C*, *Fig. 11*, containing many turns of thin insulated wire and to insert the same in the path of the current to the plate. When I connect one of the ends of the coil to the wire leading to another insulated plate *P*, and its other end to the terminal *T*, of the induction coil, and set the latter to work, a current passes through the inserted coil *C*, and the existence of the current may be made manifest in various ways. For instance, I insert an iron core *i*



Showing effects of currents flowing through open circuits.

within the coil. The current being one of the very high frequency, will, if it be of some strength, soon bring the iron core to a noticeably higher temperature, as the hysteresis and current losses are great with such high frequencies.

One might take a core of some size, laminated or not, it would matter little; but ordinary iron wire one-sixteenth or one-eighth of an inch thick is suitable for the purpose. While the induction coil is working, a current traverses the inserted coil and only a few moments are sufficient to bring the iron wire *i*, to an elevated temperature sufficient to soften the sealing wax *s*, and cause a paper washer *p*, fastened by it to the iron wire to fall off. But with the apparatus, such as I have here, other much more interesting demonstrations of this kind can be made. I have a secondary *S*, *Fig. 12*, of coarse wire wound upon a coil similar to the first. In the preceding experiment the

current through the coil *C*, *Fig. 11*, was very small, but there being many turns, a strong, heating effect was, nevertheless, produced in the iron wire. Had I passed that current through a conductor in order to show the heating of the latter, the current might have been too small to produce the effect desired. But with this coil provided with a secondary winding, I can now transform the feeble current of high tension which passes through the primary *P*, into a strong secondary current of low tension, and this current will certainly do what I expect. In a small glass tube (*t*, *Fig. 12*), I have enclosed a coiled platinum wire *w*, merely in order to protect the wire. On each end of the glass tube is sealed a terminal of stout wire, to which one of the ends of the platinum wire *w* is connected. I join the terminals of the secondary coil to these terminals and insert the primary *p*, between the insulated plate P_1 and the

Fig. 12

Conversion on open circuit with coil and insulated plate.

terminal T_1 of the induction coil, as before. The latter being set to work, the platinum wire *w*, is instantly rendered incandescent and can be fused even if it be very thick.

Instead of the platinum wire I now take an ordinary fifty-volt sixteen candle-power lamp. When I set the induction coil in operation, the lamp filament is brought to high incandescence. It is, however, not necessary to use the insulated plate, for the lamp (*l*, *Fig. 13*) is rendered incandescent even if the plate P_1 be disconnected. The secondary may also be connected to the primary as indicated by the dotted line in *Fig. 13*, to do away more or less with the electrostatic induction, or to modify the action otherwise.

I may here call attention to a number of interesting observations with the lamp. First, I disconnect one of the terminals of the lamp from the secondary *S*. When the induction coil plays, a glow is noted which fills the whole

bulb. This glow is due to eleotrostatic induction. It increases when the bulb is grasped with the hand, and the capacity of the experimenter's body thus added to the secondary circuit. The secondary, in effect, is equivalent to a metallic coating, which would be placed near the primary. If the secondary, or its equivalent, the coating, were placed symmetrically to the primary, the electrostatic induction would be *nil* under ordinary conditions; that is, when a primary return circuit is used, as both halves would neutralize each other. The secondary *is* in fact placed symmetrically to the primary, but the action of both halves of the latter, when only one of its ends is connected to the induction coil, is not exactly equal; hence, electrostatic induction takes place, and hence the glow in the bulb. I

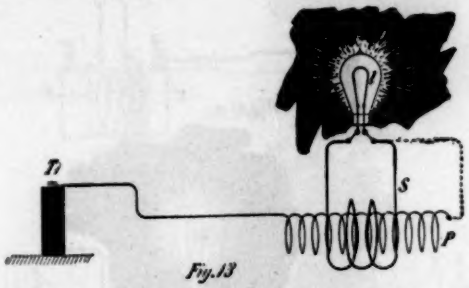


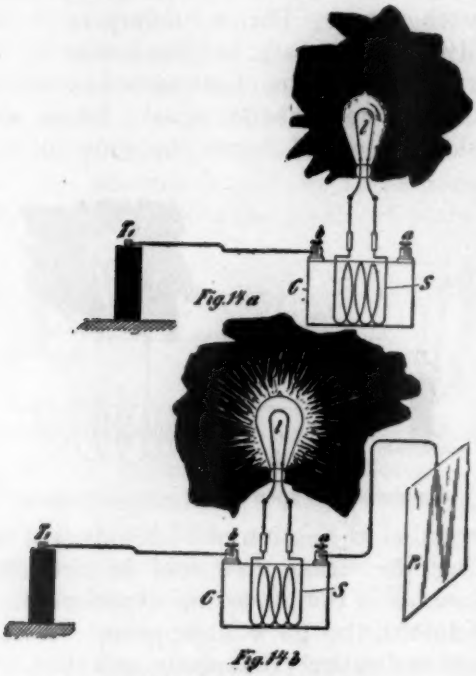
Fig. 13

Conversion on open circuit with coil alone.

can nearly equalize the action of both halves of the primary by connecting the other, free, end of the same to the insulated plate, as in the preceding experiment. When the plate is connected, the glow disappears. With a smaller plate it would not entirely disappear, and then it would contribute to the brightness of the filament when the secondary is closed, by warming the air in the bulb.

To demonstrate another interesting feature, I have adjusted the coils used in a certain way. I first connect both the terminals of the lamp to the secondary, one end of the primary being connected to the terminal T_1, of the induction coil and the other to the insulated plate P_1, as before. When the current is turned on, the lamp glows brightly, as shown in *Fig. 14b*, in which C is a fine wire coil

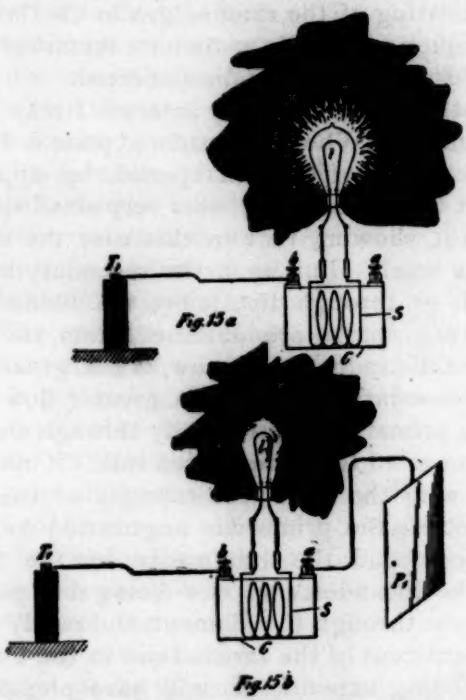
and S a coarse wire secondary wound upon it. If the insulated plate P_1 be disconnected, leaving one of the ends a of the primary insulated, the filament becomes dark or generally it diminishes in brightness (*Fig. 14a*). Connecting again the plate P_1, and raising the frequency of the current I make the filament quite dark or barely red (*Fig. 15b*). Once more I will disconnect the plate. One will of course infer that when the plate is disconnected, the current



Effect of attached plate with low frequencies.

through the primary will be weakened, that, therefore, the E. M. F. will fall in the secondary S, and that the brightness of the lamp will diminish. This might be the case and the result can be secured by an easy adjustment of the coils; also by varying the frequency and potential of the currents. But it is perhaps of greater interest to note, that the lamp increases in brightness when the plate is disconnected (*Fig. 15a*). In this case all the energy the primary receives

is now sunk into it, like the charge of a battery in an ocean cable, but most of that energy is recovered through the secondary and used to light the lamp. The current traversing the primary is strongest at the end *b*, which is connected to the terminal T_1, of the induction coil, and diminishes in strength toward the remote end *a*. But the dynamic inductive effect exerted upon the secondary *S* is now greater than before when the suspended plate was



Effect of attached plate with high frequencies.

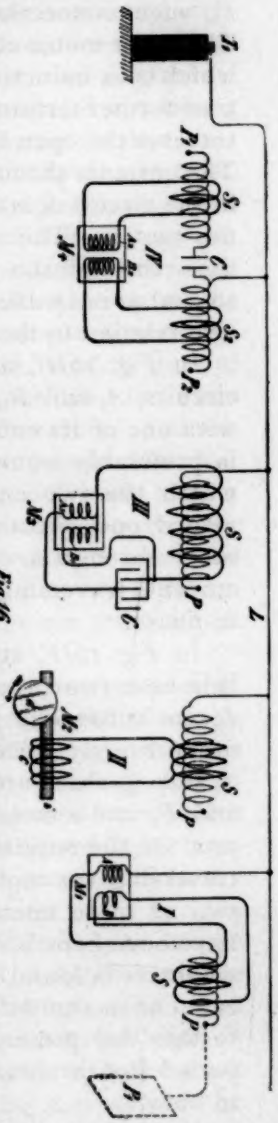
connected to the primary. These results might have been produced by a number of causes. For instance, the plate P_1, being connected, the reaction from the coil *C*, may be such as to diminish the potential at the terminal T_1, of the induction coil, and therefore weaken the current through the primary of the coil *C*. Or, the disconnecting of the plate may diminish the capacity effect with relation to the primary of the latter coil, to such an extent that the current

through it is diminished, though the potential at the terminal T_1, of the induction coil may be the same or even higher. Or, the result might have been produced by the change of phase of the primary and secondary currents and consequent reaction. But the chief determining factor is the relation of the self-induction and capacity of coil C and plate P_1, and the frequency of the currents. The greater brightness of the filament in *Fig. 15a* is, however, in part due to the heating of the rarefied gas in the lamp by electrostatic induction, which as before remarked, is greater when the suspended plate is disconnected.

Still another feature of some interest I may here bring to your attention. When the insulated plate is disconnected and the secondary of the coil opened, by approaching a small object to the secondary, only very small sparks can be drawn from it, showing that in this case the electrostatic induction is small. But upon the secondary being closed upon itself, or through the lamp, the filament glowing brightly strong sparks are obtained from the secondary. The electrostatic induction is now much greater, because the closed secondary determines a greater flow of current through the primary and principally through that half of it which is connected to the induction coil. If now the bulb be grasped with the hand, the capacity of the secondary with reference to the primary is augmented by the experimenter's body and the luminosity of the filament is increased, the incandescence now being due partly to the flow of current through the filament, and partly to the molecular bombardment of the rarefied gas in the bulb.

The preceding experiments will have prepared one for the next following results of interest, obtained in the course of these investigations. Since I can pass a current through an insulated wire merely by connecting one of its ends to the source of electrical energy; since I can induce by it another current, magnetize an iron core, and in short, perform all operations, as though a return circuit were used, clearly I can also drive a motor by the aid of only one wire. On a former occasion I described a simple form of motor comprising a single exciting coil, an iron core and

disc. *Fig. 16* illustrates a modified way of operating such an alternate current motor by currents induced in a transformer connected to one lead, and several other arrangements of circuits for operating a certain class of alternate motors founded on the action of currents of differing phase. In view of the present state of the art it is thought sufficient to describe these arrangements in a few words only. The diagram, *Fig. 16II*, shows a primary coil *P*, connected by one of its ends to the line *L*, leading from a high tension transformer terminal T_1. In inductive relation to this primary *P*, is a secondary *S*, of coarse wire in the circuit of which is a coil *c*. The currents induced in the secondary energize the iron core *i*, which is preferably, but not necessarily, subdivided, and set the metal disc *d*, in rotation. Such a motor M_2, as diagramatically shown in *Fig. 16II*, has been called a "magnetic lag motor," but this expression may be objected to by those who attribute the rotation of the disc to eddy currents circulating in minute paths when the core *i* is finally subdivided. In order to operate such a motor effectively on the plan indicated, the frequencies should not be too high, not more than 4,000 or 5,000, though the rotation is produced even with 10,000 per second, or more.



Ways of operating motors with only one lead.

Fig. 16.

In *Fig. 16I*, a motor M_1, having two energizing circuits, *A* and *B*, is diagrammatically indicated. The circuit *A* is

connected to the line L, and in series with it is a primary P, which may have its free end connected to an insulated plate P_1, such connection being indicated by the dotted lines. The other motor circuit B is connected to the secondary S, which is in inductive relation to the primary P. When the transformer terminal T_1 is alternately electrified, currents traverse the open line L and also circuit A and primary P. The currents through the latter induce secondary currents in the circuit S, which pass through the energizing coil B of the motor. The currents through the secondary S, and those through the primary P, differ in phase 90°, or nearly so, and are capable of rotating an armature placed in inductive relation to the circuits A and B.

In *Fig. 16III*, a similar motor M_3, with two energizing circuits, A_1 and B_1, is illustrated. A primary P, connected with one of its ends to the line L, has a secondary S, which is preferably wound for a tolerably high E. M. F., and to which the two energizing circuits of the motor are connected, one directly to the ends of the secondary and the other through a condenser C, by the action of which the currents traversing the circuit A_1 and B_1 are made to differ in phase.

In *Fig. 16IV*, still another arrangement is shown. In this case two primaries P_1 and P_2 are connected to the line L, one through a condenser C of small capacity, and the other directly. The primaries are provided with secondaries S_1 and S_2, which are in series with the energizing circuits A_2 and B_2, and a motor M_3, the condenser C again serving to produce the requisite difference in the phase of the currents traversing the motor circuits. As such phase motors with two or more circuits are now well-known in the art, they have been here illustrated diagrammatically. No difficulty whatever is found in operating a motor in the manner indicated or in similar ways, and although such experiments up to this day present only scientific interest, they may at a period not far distant, be carried out with practical objects in view.

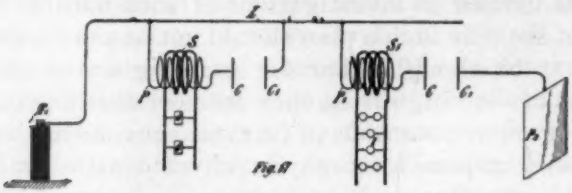
It is thought useful to devote here a few remarks to the subject of operating devices of all kinds by means of only

one leading wire. It is quite obvious, that when high frequency currents are made use of, ground connections are —at least when the E. M. F. of the currents is great—better than a return wire. Such ground connections are objectionable with steady or low frequency currents on account of destructive chemical actions of the former and disturbing influences exerted by both on the neighboring circuits; but with high frequencies these actions practically do not exist. Still, even ground connections become superfluous when the E. M. F. is very high, for soon a condition is reached, when the current may be passed more economically through open than through closed conductors. Remote as might seem an industrial application of such single wire transmission of energy to one not experienced in such lines of experiment, it will not seem so to any one who for some time has carried on investigations of such nature. Indeed, I cannot see why such a plan should not be practicable. Nor should it be thought that for carrying out such a plan currents of very high frequency are necessarily required, for just as soon as potentials of (say) 30,000 volts are used, the single wire transmission may be effected with low frequencies, and experiments have been made by me from which these inferences are made.

When the frequencies are very high it has been found in laboratory practice quite easy to regulate the effects in the manner shown in diagram, *Fig. 17*. Here two primaries P and P are shown, each connected with one of its ends to the line L, and with the other end to the condenser plates C and C', respectively. Near these are placed other condenser plates C_1 and C'_1, the former being connected to the line L and the latter to an insulated larger plate P_2. On the primaries are wound secondaries S and S_1, of coarse wire, connected to the devices m and d, respectively. By varying the distances of the condenser plates C and C', and C_1, and C'_1, the currents through the secondaries S and S_1 are varied in intensity. The curious feature is the great sensitiveness, the slighest change in the distance of the plates producing considerable variations in the intensity or strength of the currents. The sensitiveness may be

rendered extreme by making the frequency such, that the primary itself without any plate attached to its free end satisfies, in conjunction with the closed secondary, the condition of resonance. In such condition an extremely small change in the capacity of the free terminal produces great variations. For instance, I have been able to adjust the conditions so that the mere approach of a person to the coil produces a considerable change in the brightness of the lamps attached to the secondary. Such observations and experiments possess, of course, at present, chiefly scientific interest, but they may soon become of practical importance.

Very high frequencies are, of course, not practicable with motors on account of the necessity of employing iron cores. But one may use sudden discharges of low frequency and thus obtain certain advantages of high frequency currents



Single wire conversion and distribution, with simple means for regulating the effects.

without rendering the iron core entirely incapable of following the changes and without entailing a very great expenditure of energy in the core. I have found it quite practicable to operate alternating current motors with such low frequency disruptive discharges of condensers. A certain class of such motors which I described a few years ago, which contain closed secondary circuits will rotate quite vigorously when the discharges are directed through the exciting coils. One reason that such a motor operates so well with these discharges is that the difference of phase between the primary and secondary currents is 90°, which is generally not the case with harmonically rising and falling currents of low frequency. It may not be without interest to show an experiment with a simple motor of this kind, inasmuch as it is commonly thought that disruptive discharges are unsuitable for such purposes. The motor is

illustrated in *Fig. 18.* It comprises a rather large iron core *i*, with slots on the top into which are embedded thick copper washers *c c*. In proximity to the core is a freely movable metal disc *D*. The core is provided with a primary exciting coil *C*, the ends *a* and *b*, of which are connected to the terminals of the secondary *s* of an ordinary transformer, the primary *P*, of the latter being connected to an alternating distribution circuit, or generator *G*, of low or moderate frequency. The terminals of the secondary *s* are attached to a condenser *C*, which discharges through an air gap *d d*, which may be placed in series or shunt to the coil *C*. When the conditions are properly chosen, the disc *D* rotates with considerable effort and the iron core *i* does not get very perceptibly hot. With currents from a high frequency alternator, on the contrary, the core gets rapidly hot and

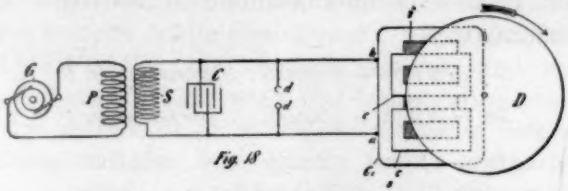


Fig. 18

Operating a motor by disruptive discharges.

the disc rotates with a much smaller effort. To perform the experiment properly it should be first ascertained that the disc *D* is not set in rotation when the discharge is not occurring at *d d*. It is preferable to use a large iron core and a condenser of large capacity, so as to bring the superimposed quicker oscillation to a very low pitch or to do away with it entirely. By observing certain elementary rules I have also found it practicable to operate ordinary series, or shunt direct current motors, with such disruptive discharges, and this can be done with or without a return wire.

[*To be continued.*]

THE SPECIFIC HEATS OF THE METALS.

By Jos. W. Richards, Ph.D.,
Instructor in Metallurgy, etc., in Lehigh University.

[*A lecture delivered before the Franklin Institute, January 30, 1893.*]

[*Concluded from p. 131.*]

Kopp obtained results from iron, the average of eight experiments at 15° to 60° being 0·1120. His minimum value was 0·108, and maximum 0·114, showing here especially the roughness of his experiments and the general unreliability of his results, although the mean, in this case, comes very near the probably true value.

Naccari made experiments similar to Byström's, arriving at the formulæ:

$$S = 0{\cdot}1072 + 0{\cdot}000116\,t + 0{\cdot}00000001466\,t^2$$

$$Sm = 0{\cdot}1072 + 0{\cdot}000058\,t + 0{\cdot}00000000489\,t^2$$

The diagram shows clearly the relative position of Naccari's results.

Pionchon experimented up to 1,150° with the soft iron of "Berry," which contained no manganese or phosphorus, and only a trace of carbon and silicon. Pionchon found two points at which heat was rendered latent, about 5·3 calories being absorbed between 660° and 720°, and 6·0 calories between 1,000° and 1,050°. He gives the following formulæ for the mean specific heat from temperatures within the designated limits to zero:

$$(0^\circ \text{ to } 660^\circ)\ Sm = 0{\cdot}11012 + 0{\cdot}000025\,t + 0{\cdot}0000000547\,t^2$$

$$(660^\circ \text{ to } 720^\circ)\ Sm = 0{\cdot}57803 - 0{\cdot}001436\,t + 0{\cdot}000001195\,t^2$$

$$(720^\circ \text{ to } 1{,}000^\circ)\ Sm = 0{\cdot}218 - \frac{39}{t}$$

$$(1{,}050^\circ \text{to } 1{,}160^\circ)\ Sm = 0{\cdot}19887 - \frac{23{\cdot}44}{t}$$

It will be seen that the first formula is of the same nature as the preceding ones, the subsequent ones are complicated

by having to include the heat rendered latent. The values of the true specific heat at any temperature are best seen by reference to the diagram. It increases gradually up to the first critical point, after which it remains constantly 0·218 until the second critical point is reached, after which it is again constant at 0·1989.

Pionchon remarks that these critical points coincide with sudden changes in the magnetic and conductive properties of iron, and also calls attention to the fact that from 720° to 1,000° the specific heat is almost exactly double what it is at zero.

The presence of carbon and silicon in iron increases its specific heat, so that steel and cast iron are superior to pure iron in calorific capacity. The study of these bodies is therefore of interest, but can only be referred to briefly here. Regnault found the specific heat of cast steel (Haussman's) 0·11848, cast iron 0·12728, white iron 0·12983. Byström found that of cast steel 0·11850, and of pig iron 0·1283, and also found the variations of these with the temperature up to 300°, for a full statement of which the reader is referred to his orginal paper.

LANTHANUM.

Dr. Hillebrand found 0·04485, using metal containing 4·6 per cent. of didymium, 1·2 per cent. of iron, 0·3 per cent. of silicon and a little aluminum, and calculating the allowance for these impurities.

LEAD.

Wilcke found 0·042; Crawford, 0·0352; Kirwan, 0·05; Dalton, 0·04, and Dulong and Petit, 0·0293; the last two by the method of cooling.

Regnault gives 0·03140 (15°–98°).

Bède deduced from his experiments up to 200° the formula:

$$Sm = 0{\cdot}0286 + 0{\cdot}000019\,t$$

This evaluated for Regnault's range gives 0·03075, two per cent. below Regnault's value.

Kopp obtained 0·0308, 0·0302, 0·0293 and 0·0302; mean 0·0301 (15°–60°). Bède's formula gives for this range 0·0300.

Person found the specific heat of molten lead (335°–430°) 0·0402.

Naccari experimented to 300°, and deduced the formulæ:

$$S = 0{\cdot}02972 + 0{\cdot}0000136\,t$$

$$Sm = 0{\cdot}02972 + 0{\cdot}0000068\,t$$

This gives for Regnault's temperatures 0·03049, even a little below Bède.

Le Verrier finally states that the specific heat of lead is constantly 0·038 between 0° and 230°, that between 220° and 250° it is almost zero, for the metal absorbs almost no heat in transversing this interval of temperature, and between 250° and 300°, the specific heat is constantly 0·0465. These results are so unexpected and differ at ordinary temperatures so greatly from Regnault's value (twenty-two per cent. difference), that we must conclude that they are quite improbable.

It is seen that there is some uncertainty as to the specific heat of lead at ordinary temperatures. On examining the diagram, it is seen that Bède's value for the specific heat increases until near the melting point it is very nearly that observed by Person in the molten state. Now, since it has been observed in other metals that the specific heat in the solid state, near the melting point, approaches the specific heat in the molten state, the writer is disposed to consider Bède's and Person's results as corroborating each other. I would, therefore, modify Bède's formula by using Regnault's value as affecting the constant, and write as the probably correct formulæ for lead:

$$Sm = 0\,02925 + 0{\cdot}000019\,t$$

$$S = 0\,02925 + 0{\cdot}000038\,t$$

This curve will pass through Regnault's value, and have the rate of increase determined by Bède.

Latent Heat of Fusion.—Dr. Irvine, Jr., determined that the latent heat would raise the temperature of an equal weight of solid lead at the melting point 162° F., and, using Crawford's value for the specific heat, this equals 5·6 calories Fahrenheit or 3·1 Centigrade. If we use the value given by

the above formula, 0·042415, his determination would give the latent heat as 3·8 calories.

Rudberg worked by the method of cooling also, and assuming that solid lead at its melting point had a specific heat of 0·0352, he obtained 5·858 calories as the latent heat. If we use 0·042415 instead of 0·0352, the latent heat would become 7·058.

Person found molten lead at its melting point to give out 15·61 calories. Assuming Regnault's value to be constant to the melting point, he obtained 5·37 calories as the latent heat. Had he used Bède's formula he would have obtained 4·27 calories, or with Naccari's formula 5·18 calories, or Bède's corrected formula 3·94. Or using the *total* amount of heat in solid lead at its melting point as found by Le Verrier (11·68 calories), the latent heat would be 3·93.

I think the best conclusion to be drawn from this discussion is that the latent heat of fusion of lead is about this latter figure, 3·95 calories.

LITHIUM.

Regnault gives 0·941 at 20°–98°.

MAGNESIUM.

Regnault found 0·25 (22°–98°); Kopp obtained results varying between 0·240 and 0·249, mean 0·245 (15°–55°).

MANGANESE.

Regnault first found 0·1441 (14°–98°), but remarked that the metal was impure. Afterwards he obtained 0·1330 with a specimen which showed important quantities of carbon and silicon. Still later he found 0·122 (12°–98°) for a specimen containing still a little silicon.

MERCURY.

Crawford found 0·0357; Leslie, 0·0290, and Kirwan, 0·033. Dulong and Petit found 0·033 (0°–100°) and 0·035 (0°–300°), giving the formula

$$Sm = 0{\cdot}0320 + 0{\cdot}00001\,t$$

Regnault found 0·03332 (12°–98°), and 0·0319 at 59°, in the solid state.

Winkelman found 0·03312 (20°–50°), and 0·03278 (25°–142°). This last result would indicate a decrease in the specific heat with rise of temperature, and lead to the formula

$$S = 0{\cdot}0336 - 0{\cdot}0000069\ t$$

Pettersson found

0° to — 5°,	0·033266
— 5° to — 16°,	0·033262
— 5° to — 26°,	0·033300
— 5° to — 36°,	0·033299

Naccari has recently determined the following values for the true specific heat:

0°,	0·03337
50°,	0·03310
100°,	0·03284
150°,	0·03259
200°,	0·03235
250°,	0·03212

These data show a regularly *decreasing* specific heat, and lead to the formulæ

$$S = 0{\cdot}03337 - 0{\cdot}0000055\ t + 0{\cdot}000000002\ t^2$$
$$Sm = 0{\cdot}03337 - 0{\cdot}00000275\ t + 0{\cdot}00000000667\ t^2$$

It will be noticed that Naccari's figures agree well with those of Winkelman, giving for 20°–50°, 0·03322, and for 25°–142°, 0·03309. Naccari's formula gives for Regnault's temperatures (12°–98°) 0·03314, which is only 0·5 per cent. different.

Milthaler has found as the mean of his experiments the formula

$$S = S_0\,(1 - 0{\cdot}000138\ t)$$

If S_0 (the true specific heat at zero) be taken as 0·033266 (Pettersson's value) the formula becomes

$$S = 0{\cdot}033266 - 0{\cdot}0000046\ t$$

Or, if Naccari's value for S_0 is taken

$$S = 0{\cdot}03337 - 0{\cdot}0000046\ t$$

Or, if Regnault's value for S at 60° is taken

$$S = 0{\cdot}033596 - 0{\cdot}00000461\ t$$

If Pettersson's results are reliable, they show that the specific heat of mercury between 0° and 36° is practically

constant. The most recent experiments agree in showing that above 0° the specific heat decreases with the temperature.

Kundt and Warburg have determined the specific heat of vapor of mercury, which is found to be 0·1714 at constant pressure and 0·102843 at constant volume. Since these quantities are to each other in the proportion 5 : 3, it follows from the mechanical theory of heat that the molecule of mercury must consist of only one atom; a conclusion which agrees with that deduced from the density of mercury vapor, thus confirming in a remarkable manner one of the deductions from Clausius' theories.

Latent Heat of Fusion and Vaporization.—Person found the latent heat of fusion by reversing the method of cooling, as 2·84 calories. Fabre and Silberman found the latent heat of vaporization in an indirect way by means of iodine vapor as 77·5 calories.

MOLYBDENUM.

Regnault found 0·07218 (12°–98°), but the specimen contained an undetermined amount of carbon.

De la Rive and Marcet found 0·0659 at ordinary temperatures by the method of cooling.

NICKEL.

Dalton found 0·10 and Dulong 0·1035, both by the method of cooling.

Regnault found 0·10863 (13°–99°).

Pionchon investigated up to 1,050°. He found a gradual increase in the specific heat up to 230°, but between that and 400° a very rapid increase, but after 400° the former rate resumed. He, therefore, concluded that between those two temperatures there is a gradual change of state, which would represent the absorption of 4·64 calories as latent heat. The diagram shows this break clearly. Pionchon gives the following formulæ for the mean specific heat to zero from any temperature within the designated limits.

Between 0° and 230° $Sm = 0{\cdot}10836 + 0{\cdot}00002233\,t$

230° " 400° $Sm = 0{\cdot}183493 - 0{\cdot}000282\,t + 0{\cdot}0000000467\,t^2$

440° " 1,050° $Sm = 0{\cdot}099 + 0{\cdot}00003375\,t + \frac{6{\cdot}55}{t}$

Pionchon remarks that this change of state corresponds to known variations in nickel's magnetic and conducting properties.

Naccari experimented up to 320°, and deduces the formulæ:

$$S = 0{\cdot}1043 + 0{\cdot}0000946\,t$$

$$Sm = 0{\cdot}1043 + 0{\cdot}0000473\,t$$

Naccari remarks that all tests showed this metal to be pure, and that as far as he went he found no trace of the phenomenon mentioned by Pionchon.

OSMIUM.

Regnault found 0·0311 (22°–98°).

PALLADIUM.

Regnault gives 0·05928 (11°–98°).

Violle investigated it up to 1,265°, and found results agreeing to the formula:

$$Sm = 0{\cdot}0582 + 0{\cdot}000010\,t$$

This formula gives for Regnault's range 0·05929, a remarkable coincidence.

The amount of heat in solid palladium as near as possible to its melting point was found to be 109·8 calories, which, according to the above equation, would indicate a maximum fusing point of 1,500°.

Latent Heat of Fusion.—Three experiments to find the heat in molten palladium at its setting point gave 146·0, 145·8, and 146·4 calories, respectively, mean 146·1. Subtracting from this the heat in solid palladium at that temperature leaves 36·3 calories as the latent heat of fusion.

PLATINUM.

Dr. Irvine found 0·03, and Dulong and Petit 0·0314 by the method of cooling.

Dulong and Petit found 0·0335 (0°–100°) and 0·0355 (0°–300°), which lead to the formula

$$Sm = 0{\cdot}0325 + 0{\cdot}00001\,t$$

Pouillet made a careful study of platinum in 1836, using

the air thermometer up to 1,200°. The data which he gives lead to the formulæ:

$$Sm = 0.03308 + 0.0000042\ t$$
$$S = 0.03308 + 0.0000084\ t$$

Regnault found 0·03243 (12°–99°).

Byström obtained rather anomalous results. He found that up to 250° the rate of increase was uniform, according to the formulæ:

$$S = 0.032386 + 0.00000094\ t + 0.000000188\ t^2$$
$$Sm = 0.032386 + 0.00000047\ t + 0.0000000627\ t^2$$

Above 250° Byström observed a very sudden rise, which no other observer has noted. The formula would give the true specific heat at 300°, 0·034360, while Byström records 0·034750, a sudden rise of 1·2 per cent. above the curve which fits all his lower results.

Kopp obtained results varying from 0·0316 to 0·0335 (15°–60°), so that his observations are of no interest in comparisons of accurate data.

Weinhold also investigated platinum, but his results vary so greatly among themselves as to be worthless.

Violle has made a most careful study of platinum up to 1,200°. He remarks that Pouillet's observations were in error chiefly because he used a platinum bulb for his air thermometer, which gives erroneous results because of transfusion of gases through the platinum at high temperatures. Violle used a porcelain bulb, and all the precautions possible to take in such experiments. Since these determinations and formulæ are so greatly relied on in using platinum as a pyrometer, I have considered it important that Violle's results should be thoroughly understood. The diagram of Violle's results shows the particular values found by him for the mean specific heat to zero, and their proximity to the line which he chooses as best representing his results. The greatest difference between his experiments was about 1·1 per cent. at any given temperature, the greatest deviation from the assumed mean line 0·8 per cent.

$$Sm = 0.0317 + 0.000006\ t$$

Violle found by direct experiment *Sm* (0°–100°) 0·0323. which would become 0·0324 for Regnault's range of temperatures, results practically identical. We must regard Violle's results as being altogether the best we possess for platinum.

Latent Heat of Fusion.—Violle made five determinations of the heat in solid platinum as near to its melting point as possible, the mean value obtained being 75·21 calories to zero. (By the formula this would indicate a maximum point of fusion of 1,775°.) Four experiments were made on the heat in molten metal at its setting point, the mean value obtained being 102·39 calories to 0°. From these data the latent heat of fusion is 27·18 calories.

POTASSIUM.

Regnault found 0·1660 between — 78° and + 10°.

RHODIUM.

A specimen containing iridium gave Regnault 0·0553 (12°–98°); another purer specimen 0·0580.

RUTHENIUM.

Bunsen found 0·0611 (0°–100°).

SILVER.

Wilcke gave 0·082; Dalton, 0·08.

Dulong and Petit found 0·0557 (0°–100°) and 0·0611 (0°–300°), which would give the formula of two terms.

$$Sm = 0{\cdot}0530 + 0{\cdot}000027\ t$$

Regnault obtained 0·05701 (14°–99°), almost two per cent. higher than given by the above formula.

Byström investigated up to 300° and gives his results calculated for the true specific heats at every 50° up to 300°. His figures are represented by the equations

$$S = 0{\cdot}05698 + 0{\cdot}0000023\ t + 0{\cdot}000000032\ t^2$$

$$Sm = 0{\cdot}05698 + 0{\cdot}00000115\ t + 0{\cdot}0000000107\ t^2$$

This equation would give for Regnault's temperatures 0·05725, about identical with Regnault's value.

Kopp obtained values varying from 0·0552 to 0·0574, mean 0·0560 (15°–65°).

Bunsen obtained 0·0559 (0°–100°), a value almost identical with Dulong and Petit's.

Pionchon investigated up to 1,020°. He found up to the melting point a regular formula of three terms, viz:

$$Sm = 0{\cdot}05758 + 0{\cdot}0000044\ t + 0{\cdot}000000006\ t^2$$

This equation gives for Regnault's range of temperature 0·05815, which is two per cent. above his value. In the molten state, Pionchon found the following formula for the mean specific heat to zero:

$$Sm = 0{\cdot}0748 + \frac{17{\cdot}20}{t}$$

the true specific heat in this condition being constantly 0·0748. The formula for the solid state would give its true specific heat at the melting point as 0·075: we thus have another example of the specific heat in the solid state approaching equality to that in the liquid state as the melting point is approached.

Naccari's experiments up to 300° led to the following formulæ:

$$S = 0{\cdot}05449 + 0{\cdot}0000214\ t$$

$$Sm = 0{\cdot}05449 + 0{\cdot}0000107\ t$$

The latter, evaluated for Regnault's temperatures, gives 0·0557, which is almost identical with Dulong and Petit's value.

Le Verrier claims that the specific heat is constant up to 260°, then changes suddenly and is again constant up to 660°, then changes again and is constant to the melting point. He gives between 0° and 260°, 0·0565; between 260° and 660°, 0·075, between 660° and 900°, 0·066. As no other observer has observed any such sudden changes, it is probable that Le Verrier is mistaken in his results. Le Verrier states that silver contains sixty-two calories of heat towards 930°, a little before the fusion. Pionchon places the fusing point at 907°, and gives 60·32 calories as the amount of heat in the metal at this temperature.

It is difficult to decide just where the truth lies in this case of the specific heat of silver. If it were allowable to do so, an average of all the results at ordinary tempera-

tures would come close to Regnault's value. But, since Pionchon made no experiments below 400°, I should be disposed to place less reliance on his values for temperatures from 0° to 100°. I would suggest for use below 400°, a formula based on the mean of Regnault's and Bunsen's observations at 0° to 100° and on Pionchon's result at 0° to 400°. Such a formula would be

$$S = 0{\cdot}0555 + 0{\cdot}00001886\ t$$
$$Sm = 0{\cdot}0555 + 0{\cdot}00000943\ t$$

This gives for Bunsen's range 0·05644, for Regnault's range 0·0566, being in each case about one per cent. from their figures. At 220°, the point where it will be seen from the diagram that the lines of Byström and Naccari meet, at the value for S of 0·0590, the above formula gives 0·0596. At 400° the line joins in with Pionchon's curve, and above 400° Pionchon's formula may be assumed as probably accurate. The curve proposed above is given as a dotted line on the diagram.

Latent Heat of Fusion.—Person determined this by the method of cooling to be 21·07 calories. Pionchon's formula for solid silver evaluated for the melting point gives 60·32, and his formula for liquid silver evaluated for that temperature gives 85·04, the difference between these would therefore be the latent heat of fusion, or 24·72 calories. Pionchon's number is probably the nearest right.

SODIUM.

Regnault obtained 0·293 (38° to + 10°).

THALLIUM.

Regnault obtained 0·0335 (18°–98°).

THORIUM.

Nilson obtained 0·0276 (0°–100°).

TIN.

Crawford found 0·0704; Wilcke, 0·06, and Kirwan, 0·068. Dalton obtained 0·07 by the method of cooling.

Dulong and Petit obtained 0·0514 by the method of cooling.

Regnault found for impure English tin 0·05695 (16°–97°), and for pure Banca tin 0·05623 (14°–99°).

Bède obtained results up to 213°, from which he deduced the formula:

$$Sm = 0{\cdot}0500 + 0{\cdot}000044\,t$$

This evaluated for Regnault's temperatures gives 0·0550, over two per cent. lower.

Kopp obtained results varying between 0·0493 and 0·0553, mean 0·0531 (15°–65°) a result of very little value. A second set of experiments gave him results between 0·0543 and 0·0573, mean 0·0565. The mean of the two sets of experiments is 0·0548; but, the highest and lowest values he obtained were 16 per cent. apart!

Person found the specific heat of molten tin to be 0·0637, (250°–340°).

Bunsen found for *allotropic* tin 0·0545 (0°–100°) and for cast tin 0·0559 (0°–100°).

Pionchon found for molten tin up to 1,000° the formula:

$$Sm\ (\text{to } 0°) = 0{\cdot}0612931 - 0{\cdot}0000104741\,t + 0{\cdot}00000000103448\,t^2 + \frac{14{\cdot}375}{t}$$

The writer has determined for Banca tin between 15° and 99°, 0·0566, a value 0·7 per cent. above Regnault's.

Latent Heat of Fusion.—Dr. Irvine, Sr., working under Dr. Black, found that the latent heat would raise the temperature of solid tin 500° F. (at what temperature not stated). Dr. Irvine, Jr., found in a similar way 507°, the mean between 495° and 520°. In reaching these results, the heat in molten tin at its setting point was first found. This was divided by the ordinary specific heat of tin, and the melting point subtracted. This would be equivalent to assuming that the heat in solid tin at its melting point is the product of that temperature into the ordinary specific heat. Since we do not know what value Irvine assumed for the latter we cannot correct his figures. If he used Crawford's value 0·0704, his result would represent about 18·3 calories; if he had used 0·0570, his results would have been 16 calories.

Rudberg worked by the method of cooling, and assuming that the specific heat of solid tin at its melting point is

0·0586, he obtained 13·31 calories as the latent heat of fusion. If we assume that the specific heat at this temperature is the same as Person found for molten tin (0·0637), Rudberg's results would become 14·46 calories.

Person found the amount of heat in molten tin at its setting point to be 27·33 calories. Then, assuming that Regnault's value for the specific heat was applicable to the melting point, he found the heat in solid tin at its melting point to be $232·7 \times 0·05623 = 13·08$ calories, which left 14·25 calories as the latent heat of fusion. Had he used the amount found by Bède in solid tin, at its melting point, by direct experiment, he would have had $27·33 - 13·60 = 13·73$ calories as the latent heat.

Pionchon's formula for liquid tin evaluated for the melting point gives 28·20 calories. Subtracting Bède's value for the heat in solid tin leaves 14·60 calories as the latent heat.

Direct experiment, by pouring liquid tin just at its melting point into water, has given the writer 28·16 calories. This, less 13·60, would leave 14·56 calories as the latent heat of fusion. The close agreement of this result with Pionchon's is worthy of notice, especially since the heat in the molten tin was determined by two entirely different methods of procedure.

TITANIUM.

Nilson and Pettersson determined the mean specific heat as follows:

0° to 100°,	0·1125
0° to 211°,	0·1288
0° to 301°·5,	0·1485
0° to 404°,	0·1620

These would give the formula

$$Sm = 0·0978 + 0·000147\ t$$

This formula fits almost exactly the first, second and last of the above observations, but gives 0·1421 for the third. A regular curve could not be found passing through all four of those values.

TUNGSTEN.

Regnault gives 0·03636 (11°–99°).

URANIUM.

Blümcke found 0·02811, 0·02762, 0·02813, mean 0·0280 (0°–99°). Zimmerman found on two specimens as a mean value 0·0275 and 0·2812 (0°–99°).

ZINC.

Wilcke found 0·102; Crawford, 0·0943, and Dalton, 0·10.

Dulong and Petit found 0·0927 (0°–100°) and 0·1015 (0°–300°). These results would give the formula:

$$Sm = 0{\cdot}0883 + 0{\cdot}000044\,t$$

Neumann obtained 0·0929, at ordinary temperatures, by the method of cooling.

Regnault obtained with commercial zinc 0·09985, 0·10049, 0·100003, and with chemically pure zinc 0·09555 (14°–99°).

Bède deduces from his experiments the formula

$$Sm = 0{\cdot}0865 + 0{\cdot}000044\,t$$

He observes that his metal contained a little lead.

Dulong and Petit's result is three per cent., and Bède's four per cent. below Regnault's.

Kopp obtained results varying between 0·0899 and 0·0977, mean 0·0932 (15°–65°).

Bunsen obtained 0·0935 (0°–100°).

Naccari deduces from his experiments up to 320° the formula:

$$S = 0{\cdot}0907 + 0{\cdot}000044\,t$$

This gives 0·0932 for Regnault's temperatures.

Le Verrier states that from 0° to 110° the specific heat of zinc is constantly 0·096; between 110° and 140° it is very variable, there being an absorption of 0·8 calorie about 110°; that from this point to 300° the specific heat is constantly 0·105, and between 300° and 400° constantly 0·122. The total amount of heat is about 46 calories at about 410°, a trifle before fusion.

Latent Heat of Fusion.—Dr. Irvine, assuming the specific heat of zinc constant to the fusing point (0·0943) found by experiment 62·5 calories in molten zinc, and subtracting the heat in solid zinc at its melting point (388°·9 × ·0943), he

obtained 62·5 — 35·7 = 26·8 calories as the latent heat. (I have transferred his Fahrenheit degrees to Centigrade.)

Person obtained an average of 67·81 calories in molten zinc at its setting point, and assuming Regnault's value for the specific heat true to the melting point, he found the latent heat to be 67·81 — (415·3 × 0·09555) = 67·81 — 39·68 = 28·13 calories. However, we know that this latter assumption is incorrect.

Supposing we take for the mean specific heat a line which would pass through Regnault's result and be parallel to Dulong and Petit's and Bède's lines, which are parallel to each other. Such a line would be

$$Sm = 0{\cdot}09058 + 0{\cdot}000044\, t$$

Now, Dulong and Petit's formula would give for the heat in solid zinc at its melting point 44·2 calories, Bède's 43·5 calories, and Naccari's 40·4 calories. The above formula gives 45·2 calories. It is noticeable that when Le Verrier gives the total heat in any metal close to its melting point, his value is usually near to the best determinations. He says there is about forty-six calories in zinc just below its melting point. I should, therefore, take the value 45·2 given by the above equation as probably the nearest right.

Irvine's result will now become 62·5 — 45·2 = 17·3 calories. Person's result, 67·81 — 45·2 = 22·61 calories. Person's is, of course, the better determination of the two.

ZIRCONIUM.

Mixter and Dana have obtained 0·0662 (0°-100°) with the ice calorimeter.

LITERATURE OF THE SPECIFIC HEATS OF THE METALS.

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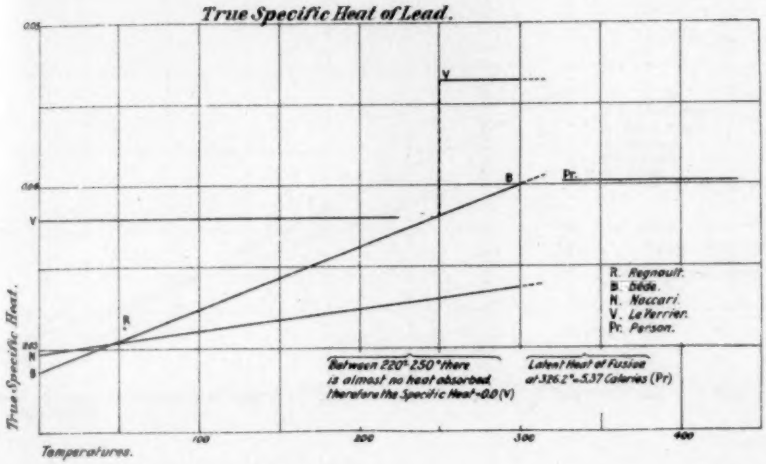
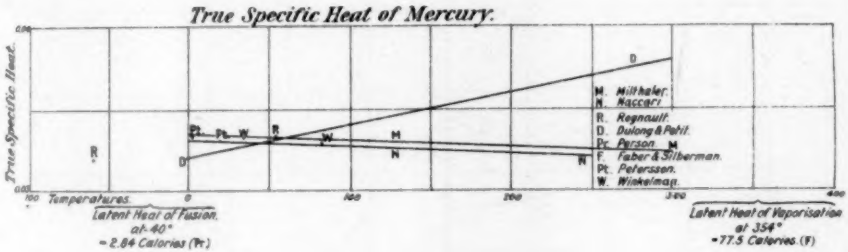
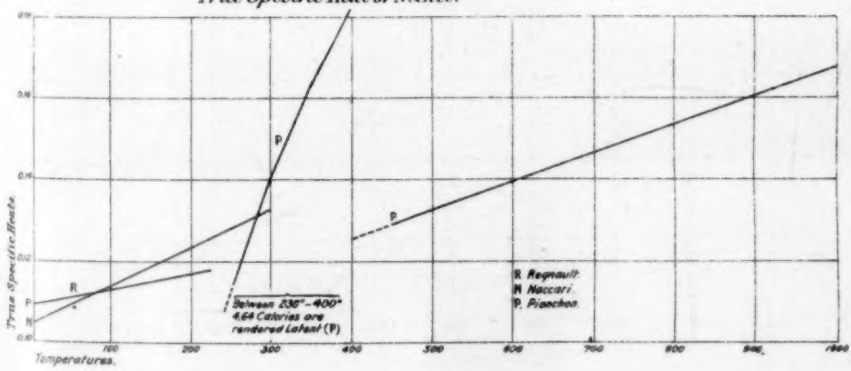

True Specific Heat of Lead.
True Specific Heat.
Temperatures.
V
Pr.
B
R
N
S
R. Regnault.
B. Bède.
N. Naccari.
V. LeVerrier.
Pr. Person.
Between 220° 250° there is almost no heat absorbed therefore the Specific Heat=0.0 (V)
Latent Heat of Fusion at 326.2°=5.37 Calories (Pr)
100
200
300
400


True Specific Heat of Mercury.
True Specific Heat.
Temperatures.
R
D
Pt. Pe. W.
M
N
M. Hilthaler.
N. Naccari.
R. Regnault.
D. Dulong & Pehl.
Dr. Person. M.
F. Faber & Silberman.
Pt. Petersson.
W. Winkelmann.
Latent Heat of Fusion. at -40° = 2.84 Calories (Pr.)
Latent Heat of Vaporisation at 354° =77.5 Calories. (F)
-100
0
100
200
300
400


True Specific Heat of Nickel.
True Specific Heats.
Temperatures.
P
R
N
Between 230°-400° 4.64 Calories are rendered Latent (P)
R. Regnault.
N. Naccari.
P. Pioechon.
100
200
300
400
500
600
700
800
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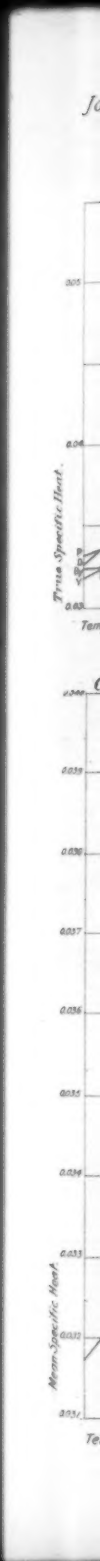

Mean Specific Heat.

True Specific Heat of Platinum.

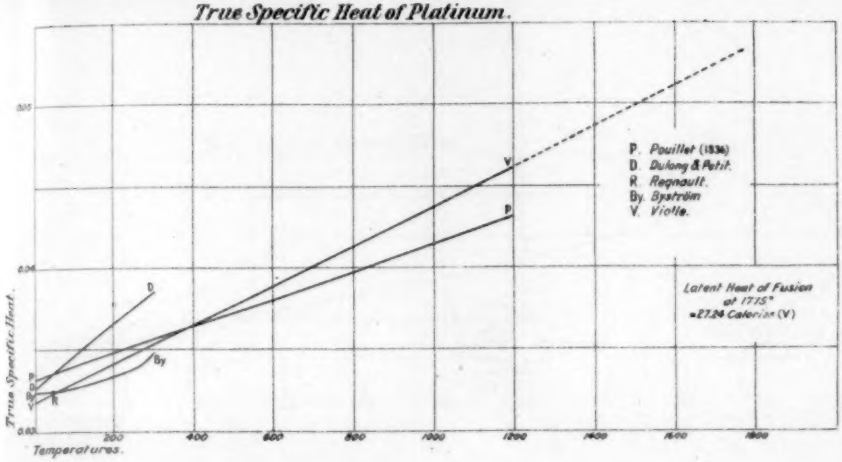
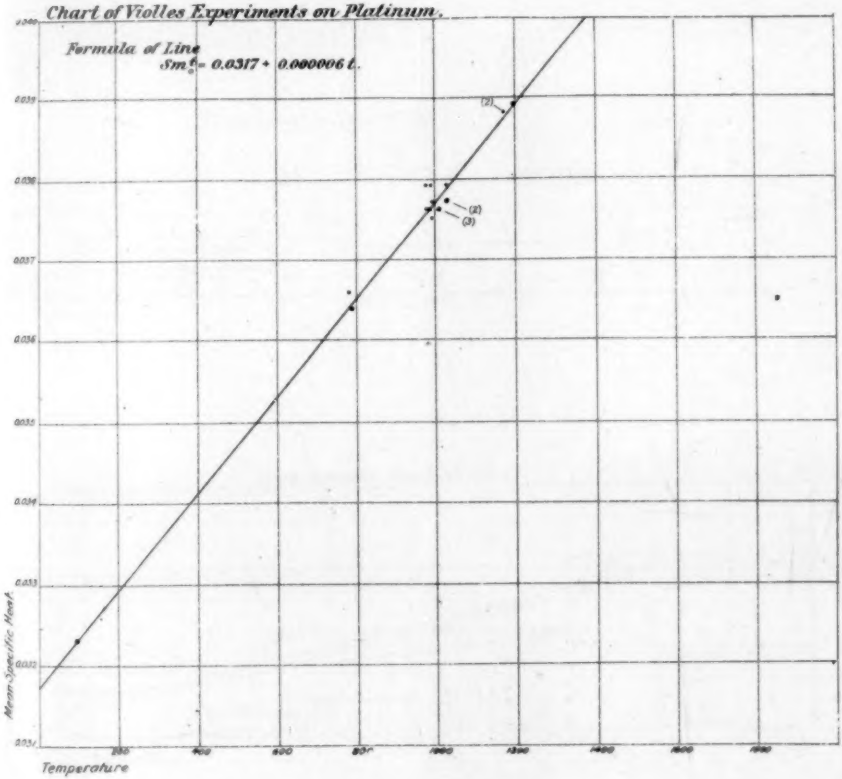


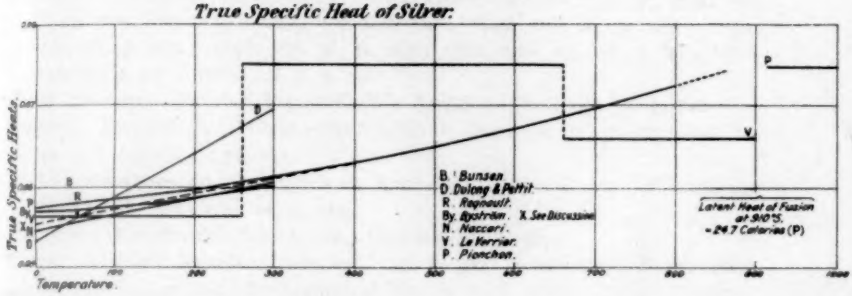
Chart of Violles Experiments on Platinum.



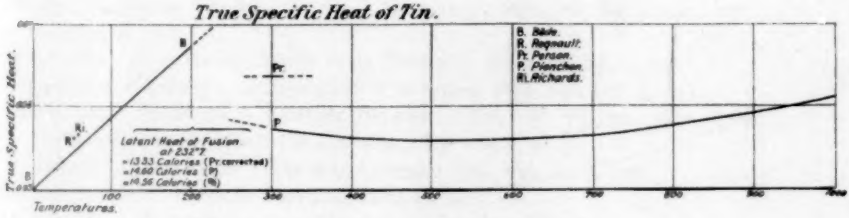
True Specific Heats.

True Specific Heat.

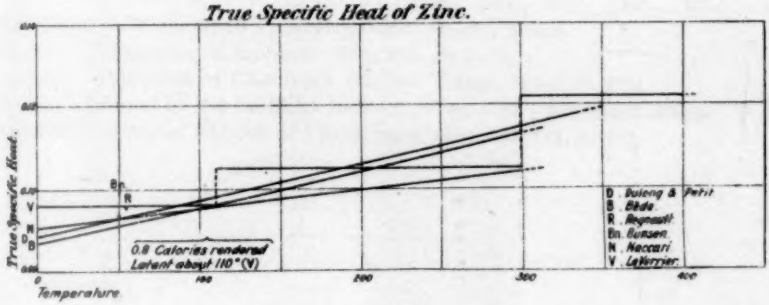
True Specific Heat of Silver.



True Specific Heat of Tin.



True Specific Heat of Zinc.



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CARBORUNDUM: ITS HISTORY, MANUFACTURE AND USES.

By E. G. Acheson.

[*Read at the stated meeting of the Institute, held June 21, 1893.*]

For a number of years prior to 1890, I had been keeping a constant watch for anything that might suggest a method by which carbon could be crystallized. A realization of the importance of abrasive materials, in the industrial arts, together with the known superiority of crystalline carbons over all other substances, acted as a constant stimulus to continued exertion for the solution of the problem. In the year mentioned, having become associated with an electric light company, at Monongahela, Pa., I found myself in position to conduct experiments on a line which I had some years earlier formulated. The scheme was to cause carbon to be dissolved in melted silicate of alumina, or in the metals reduced therefrom, and by cooling the same to the point of solidification, cause the contained carbon to crystallize. You have heard that "fools rush in where angels fear to tread," and had I been a chemist, it is probable that such an experiment would not have been thought worthy of consideration, and certainly would not have been attempted. Be this as it may, the experiments were made with results more or less satisfactory.

The first experiment was with a furnace constructed of an iron bowl lined with carbon, in the central cavity of which was placed a mixture of carbon and clay; through the mixture was passed an electric current of sufficient amount to fuse the mass—a violent reaction following the fusion—the iron bowl, and a rod of carbon suspended in the centre of the mixture, forming the electrodes. After the mass had cooled, it was removed, broken and carefully examined, when a few bright crystals, blue in color and apparently very hard, were found to be in that part which immediately

surrounded the carbon electrodes. They were exceedingly small and only served to convince me that more and better arranged experiments would produce the desired results.

The iron bowl furnace was replaced with one constructed of refractory bricks, its interior dimensions being 10 inches in length by 4 inches wide and 4 inches deep. Into each end extended a carbon rod, so arranged as to be movable in the direction of its length, thus allowing of a variation of the distance through which the current would have to traverse the charging mixture. The electrical apparatus was arranged for supplying and regulating a current of from 100 to 200 ampères, and of an adjustable potential of from one to fifty volts. The current used was an alternating one, and the action and effects produced were thereby reduced to one of temperature, the probability of electrolytic effects not existing.

The first experiments were confined, more or less closely, to the original lines, as defined by the theory with which I commenced—carbon and clay forming the basis of operation. It was soon discovered that the crystals were not what I had expected to obtain—they were not pure carbon; in color they were usually blue, and their hardness was found to be sufficiently great to abrade the diamond. They seemed, however, to be exceedingly brittle, and it was only in the form of a very fine powder that they were capable of performing this work. Repeated attempts to charge the surface of a copper, or other metallic disc, were productive of failures, as compared with the results obtained with crushed diamond under similar conditions; the crystals being too brittle to withstand the pressure necessary to embed them, or in case of their being embedded, they would be broken and crushed when subjected to use in the manner of the well-known diamond saw.

At this point in the work the want of a name for the new product was felt. It had not been analyzed, and I was led to believe, from the materials used, together with the color, sapphire blue and ruby-red, hardness and general form, that the material was composed of carbon and alumina, and in this belief it was decided to construct the name of the new

material out of carbon and corundum, and it was named carborundum. The fitness of the name, in the eyes of the chemist, is, in view of the now known composition of the substance, doubtful, while in commerce, although phonetic and of pleasing effect in print, it is, perhaps, a trifle lengthy.

I had not continued my experiments long, before certain conditions and results obtained, led me to think the silica present in the furnace played a very prominent part in determining the quality and quantity of the carborundum; and with a view of determining whether or not this was the case, a mixture was made composed of carbon, silica and chloride of sodium; my former experiments having proven the presence of common salt to be beneficial, facili-

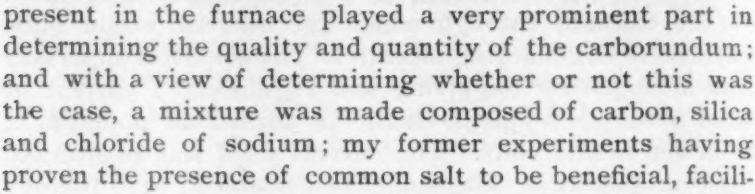

FIG. 1.—Longitudinal section of furnace.

tating the fusion, and to some extent protecting the mass in the furnace from oxidation by air contact. The carbon used was obtained from the carbon rods used in arc lamps, they having been reduced to a powder. These carbons had been made from the coke residue obtained in the distillation of petroleum. The silica was of a good grade of glass sand, its composition not being known. The salt was a good average quality. These materials were mixed in the proportion of forty per cent. each, of carbon and sand, and twenty per cent. of salt, and the mixture was placed in the furnace.

I had met with considerable trouble in operating the furnace from the difficulty in starting the fusion of the mixture, and the continual changes that occurred in the

internal resistance; the contacts, or connection between the electrodes and mixture, were also very difficult to maintain. To obviate or reduce these objectionable features, the connections and chargings were made in the manner shown in the diagram, where *A* represents the walls of the furnace, *B B* the mixture of carbon, salt and sand, *E E* the carbon rods or electrodes, and *D* a core of granular carbon, this core being enlarged at its extremities and made to surround the ends of the electrodes.

The furnace having been prepared in this manner, the electric current was made to pass. The value of this current was never constant, thus: on starting it would have a value of about forty ampères, with a potential of about fifty volts; the ampères would immediately increase, the potential being kept constant until the limit of the apparatus, or 200 ampères, had been reached, at which point the regulation of the potential would begin, finally stopping at about twenty-five volts, the ampères remaining at 200. These changes were produced, on the part of the current quantity, by a reduction in the resistance of the furnace due to the increasing temperature of the core and mixture, fusing of the salt, and eventually to the formation of a black material, consisting of a mixture of carborundum and free carbon, this material having a high conductivity. The reduction of the potential was produced by the ordinary methods of regulation, and was made to prevent the continued increase of the ampères and consequent burning out of the generating apparatus.

An experiment was made with the mixture, arrangements of charge and current as stated, the time required being about four hours. The mass heated slowly, the first outward indication of the internal heat and reactions being the appearance of monoxide of carbon, which was lighted and continued to burn during the remainder of the experiment; later, occasional bursts of white vapors of chloride of sodium would occur, and these would be followed by a flow of melted chloride of sodium, forming miniature volcanic craters.

The action was continued until there was a cessation of

these disturbances, until the blue flames of burning monoxide of carbon became tinged with a deep yellow from the sodium, and until the reduction of the potential indicated a low resistance, resulting from the formation of the black substance above referred to; the current was then stopped, and the furnace permitted to cool and opened.

A representation of the cross-section of the furnace when opened is shown in *Fig. 2*, where *A* represents the walls of the furnace; *B*, the mixture of carbon, sand and salt, now a solid cemented mass consisting of sand and carbon held together with the fused salt; *D*, the carbon core, and *G*, *C* and *W*, zones of portions of the original mixture *B*, now transformed into materials of variable composition.

The results obtained from the first experiments with a

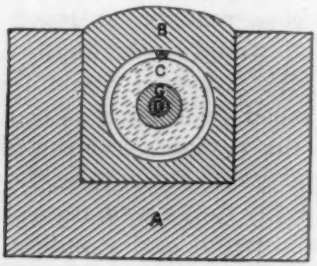

FIG. 2.—Cross-section of furnace.

mixture containing silica, instead of silicate of alumina, were so much superior to the results formerly secured, that the use of clay was abandoned.

Such is a brief sketch of the first manufacture of carborundum. Its further development and present manufacture form a large portion of the history of the Carborundum Company.

The results obtained were so promising that it was thought a lucrative business could be made of the manufacture and marketing of carborundum, and acting on this belief, a company was organized for these purposes. The processes of manufacturing carborundum and the working of it up into marketable form, were elaborated, and up to the present time are undergoing changes and improvements.

One of the first acts of the company was the equipment of a chemical laboratory, and the engagement of Dr. Otto Mulhaeuser, who entered at once into an exhaustive analytical study of carborundum. The results of his labors might be briefly stated as follows:

A sample of carborundum, as originally made from carbon, clay and salt, and having, to the unassisted eye, a blue-black color, but under the miscroscope exhibiting a few crystals of a yellow-green color, some black, some white, with the majority blue, had the following composition:

Si,	60·51
C,	30·09
Al_2O_3,	4·78
CaO,	·17
MgO,	·18
O,	4·27
	100·00

A sample of carborundum, made from carbon, sand and salt, having a clear light-green color, gave as its composition:

Si,	69·19
C,	29·71
Al_2O_3 and Fe_2O_3,	·39
CaO,	·19
MgO,	·06
O,	·47
	100·00

From these figures we learn that what we have called carborundum is, in a pure state, carbide of silicon, responding to the formula SiC, the other substances being impurities. The carbon used in the manufacture of the last sample—the one from sand—was obtained from the coke of bituminous coal, and by it the iron was introduced which we find in the analysis.

The great difficulty met with in obtaining a complete combustion of the carbon, necessitated the reduction of the sample to the finest powder: the samples used for analysis being obtained by floating in water for some minutes, thus

permitting the coarser particles to fall down and be separated from the finer. This latter portion, remaining in suspension in the water, was decanted into another vessel and permitted to settle for some longer period. The degree of fineness of the powder is then indicated by the number of minutes it stood in the first vessel. The necessity for this minute subdivision is shown by the following percentages of carbon as determined from powders of different degrees of fineness, all of them forming part of one original sample:

	Per Cent. C.
Crushed crystals,	24·82
One minute,	25·08
Two minutes,	27·06
Three minutes,	28·04
Four minutes,	29·71

The wonderful permanency of carborundum, when exposed to a high temperature, is also shown by the fact that clean and pure powder heated to bright redness in a current of oxygen for a period of one hour lost only ·41 per cent.; this same quality is also shown in the following determination of silica (SiO_2) formed by the ignition of carborundum (SiC) crystals in the open air in a platinum crucible at a bright red heat for ten hours:

	Grs.
Weight of crucible,	28·5315
Weight of crucible and SiC,	29·0362
Weight of crucible and SiC after ignition,	29·0220
Loss,	·0142
Weight after ignition,	29·0220
Weight after treating with HF and H_2SO_4,	29·0082
SiO_2,	·0138
Total loss,	·0276
Weight of SiC at first,	·5047
	Per Cent.
Total loss,	5·5
Loss per hour,	·55

While the effort is always made to produce carborundum as nearly pure as possible, the conditions of its manufacture are such as to cause a larger or smaller percentage of

impurities to be present. It is, of course, impossible to use a mixture of carbon and silicon as represented in the composition of carborundum, an excess of carbon must be present at the beginning of the process to take up the oxygen present and that entering from the air. It has been found that a very good proportion for the mixture is twenty parts carbon, twenty-five parts sand and ten parts salt (by weight). A sample of carborundum made from this mixture was tested for silica (SiO_2) and a treatment for eight days with hydrofluoric acid (HF) showed the amount present to be 1·56 per cent.

Before entering upon a consideration of the commercial features of carborundum, I will present the following condensed report of the investigations of Dr. Mulhaeuser, as prepared by him at my request for this paper.

The structure and chemical composition of the mass after the reaction was found to be as follows, reference being made to the diagram, *Fig. 2*.

D represents the path of the current, the core of carbon connecting the terminals forming with the latter the resistance by which the electrical energy is transformed into heat energy. This core shows—after the reaction—no change.

G represents a shell of a brilliant black mass which surrounds the carbon core; it consists of crystalline-like aggregates, which are arranged radially to the axis of the core. The part near the core consists of pure carbon, those parts more distant from the axis of the core are more or less mixed with carborundum crystals. A sample of that part gave, for instance, the following analytical results:

	Per Cent.
Free carbon,	66·29
Carborundum,	33·71

The free carbon has all the properties of graphite, it blackens the fingers, etc. The carborundum found in this zone *G* gave the following numbers:

	Per Cent.
C,	30·49
Si,	68·26
Fe_2O_3,	0·77
CaO,	0·48

To separate the carbon from the carborundum the mass is heated with hot oxygen. The crystals obtained in this way, though they possess generally the properties of carborundum, seem to differ a little in their optical qualities. They show, after having been freed from carbon, all the colors of the spectrum; especially brilliant are the red and violet rays.

C represents the chief product of the reaction, the zone of the carborundum, chemically spoken of as crystallized carbide of silicon. The zone of carborundum surrounds the graphite zone. The crystals are arranged radially to the axis of the core, and form in general a bright green shell. For the purpose of analysis, a sample of that shell was reduced to a powder of the proper fineness and gave the following figures:

	Per Cent.
Si,	62·70
Fe_2O_3 and Al_2O_3,	0·93
MgO,	0·11
C,	36·26

This analysis showed clearly that carborundum consists essentially of carbon and silicon associated together in the proportion of one atom each. In order to clean the product, to free it from iron, aluminum, etc., the powder was treated in the following manner: First with hydrochloric acid, then with dilute caustic soda and water, then the powder was put in a combustion tube, heated to dark redness and a current of oxygen passed over it for about one hour. All free carbon and perhaps a little of the combined carbon is burned off and a powder obtained, which contains now only silica, acid, traces of magnesia, alumina and iron, besides the carbide of silicon. By treating the powder with hydrofluoric acid, the carbide of silicon is obtained nearly pure. One of the many samples cleaned in this way, gave the following results:

	Per Cent.
Si,	69·10
C,	30·20
Al_2O_3 and Fe_2O_3,	0·49
CaO,	0·15

A compound having the formula SiC contains seventy per cent. Si and thirty per cent. C.

If pure carbon and pure silicic acid be taken the carborundum crystals will be white; if these materials contain iron, the crystals show a greenish-yellow color.

The crystals are not dissolved either by hydrofluoric acid or by any other acid; they are slightly attacked by dilute caustic and carbonated alkalies; and they are decomposed by caustic or carbonate of soda by fusion. The carbon is separated and silicon is transformed in silicic acid, the whole mass becomes black, then after some time the carbon burns off.

When reduced to a very fine powder and floated in water, carborundum does not settle, even after a period of months, It behaves like a colloidal substance, for instance metallic silver. By adding an acid or a salt, the extremely fine powder settles.

W represents a white or gray-greenish looking shell, surrounding the zone of carborundum crystals. The mass consists of small pieces of the size of the original sand grains. These pieces are soft and may be reduced very easily to a fine powder. The qualitative analysis showed that the powder had the same constituents as the carborundum crystals, and the powder was, therefore, cleaned in the same manner as in the case of those crystals. The white-green shell was crushed and alternately heated with hydrochloric acid, caustic soda, water, hot oxygen and hydrofluoric acid. The resulting powder had the following composition:

	Per Cent.
C,	27·93
Si,	65·42
Fe_2O_3 and Al_2O_3,	5·09
CaO,	0·33
MgO,	0·21

We may say then, that the powder consists essentially of carbide of silicon. But this carbide of silicon is different from the other one in that it forms no crystals, is amorphous and very soft, and of no value as an abrasive. It has been obtained at a comparatively low temperature—far distant from the core—and is identical with the product recently obtained by I. Schützenberger.

[*To be continued.*]

A NEW METHOD OF SEPARATING THE WHITE, FROM THE RED, BLOOD CORPUSCLES, BY MEANS OF THE HÆMATOKRIT.

BY JUDSON DALAND, M.D.

Instructor in Clinical Medicine and Lecturer on Physical Diagnosis and Symptomatology in the University of Pennsylvania; Assistant Physician to the University Hospital; Physician to the Philadelphia Hospital and to the Rush Hospital for Consumptives.

[*A lecture delivered before the Franklin Institute, December 9, 1892.*]

The lecturer was introduced by the Secretary of the Institute, and spoke as follows:

MEMBERS OF THE INSTITUTE, LADIES AND GENTLEMEN:

Before referring to the methods of separating the white from the red blood corpuscles, it may be of interest if I mention briefly a few of the more important facts regarding the blood.

This fluid has been the object of study by all physicians at all places, at all time; and this interest has not been confined to physicians, but has exercised its fascination over the remaining professions as well as the general public.

All through literature, both scientific and secular, constant reference is made to this wonderful liquid.

Notwithstanding the centuries that have been devoted to research in this direction but little was known of the exact composition of the blood in health and disease until the early part of the present century, and our knowledge of the existence of the red blood cells is but 220 years old.

The blood may be described as having a bright scarlet-red color in arteries, and dark bluish-red when obtained from a vein; its reaction is neutral alkaline; its odor peculiar but difficult to describe; its taste is saline; its specific gravity averages 1·055 and its temperature varies from 99° to 100° F.

The total quantity of blood is estimated as one-thirteenth of the body weight, *i. e.*, a man weighing 130 pounds contains ten pints of blood.

Broadly speaking, the blood is composed of a fluid called *plasma*, in which float small solid bodies, known as the *red* and *white* blood corpuscles.

The plasma is a liquid composed of salts and albumin dissolved in water. The red blood cell is a round body four times as broad as it is thick, concave on each side, with rounded edges and measures about $\frac{1}{3000}$ to $\frac{1}{3500}$ of an inch in diameter, and with a tendency to form rouleaux.

When viewed under the microscope it is light yellow in color, which color is due to the presence of blood pigment or hæmaglobin.

The white blood corpuscle is a spherical body, the average size of which is a trifle larger than a red blood corpuscle; it is colorless and is possessed of the curious property of throwing out processes and retracting them, which is called amæboid movement.

Some years ago an attempt was made to count the number of red and white corpuscles in each cubic millimetre of blood, and finally the Thoma-Zeiss blood counter was constructed. The instrument is composed of a capillary tube which opens into an olive-shaped bulb, containing a small glass ball about the size of a large shot. The second portion consists of a glass slide, in the centre of which is an elevated disc surrounded by a gutter. When a glass cover is placed on top a shallow cell is formed, having known dimensions. On the surface of the disc is traced a series of lines, bisecting each other in such a way as to form sixteen small squares.

By aid of the graduated pipette the blood is diluted with a two and one-half per cent. solution of potassium bichromate in the proportion of $\frac{1}{100}$. This is thoroughly mixed in the bulb of the pipette, with the aid of the glass ball, by thorough shaking. A drop of this diluted blood is then placed upon the slide, over which is placed a cover glass. The number of red cells in each small square are then counted. This number is multiplied by the dilution, and

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The white blood corpuscle is a spherical body, the average size of which is a trifle larger than a red blood corpuscle; it is colorless and is possessed of the curious property of throwing out processes and retracting them, which is called amæboid movement.

Some years ago an attempt was made to count the number of red and white corpuscles in each cubic millimetre of blood, and finally the Thoma-Zeiss blood counter was constructed. The instrument is composed of a capillary tube which opens into an olive-shaped bulb, containing a small glass ball about the size of a large shot. The second portion consists of a glass slide, in the centre of which is an elevated disc surrounded by a gutter. When a glass cover is placed on top a shallow cell is formed, having known dimensions. On the surface of the disc is traced a series of lines, bisecting each other in such a way as to form sixteen small squares.

By aid of the graduated pipette the blood is diluted with a two and one-half per cent. solution of potassium bichromate in the proportion of $\frac{1}{100}$. This is thoroughly mixed in the bulb of the pipette, with the aid of the glass ball, by thorough shaking. A drop of this diluted blood is then placed upon the slide, over which is placed a cover glass. The number of red cells in each small square are then counted. This number is multiplied by the dilution, and

this result again by 4,000, because each square is but the $\frac{1}{4000}$ of a cubic millimetre, and finally this result is divided by the number of small squares counted.

The process proved so uncertain in its results, and requires so much time and patience, that, in 1885, Professor Blix first suggested the use of centrifugal force, and later Dr. Hedin perfected this instrument, which I herewith demonstrate.

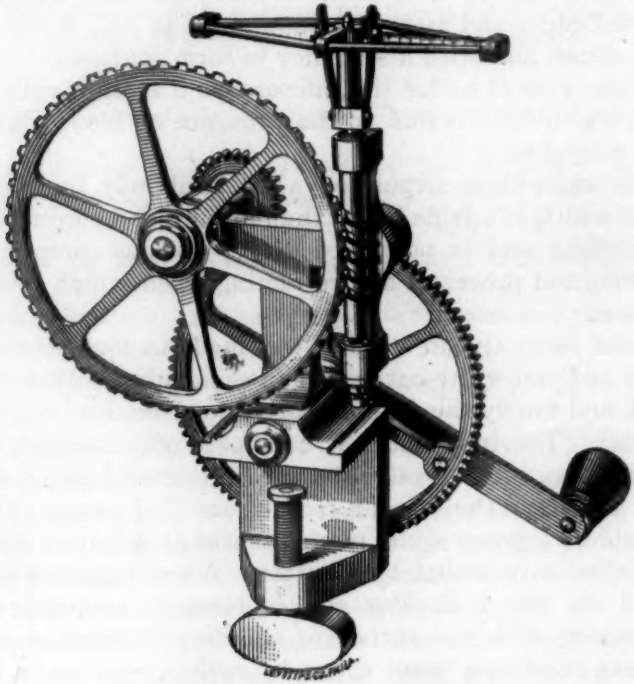


FIG. 1.—Hedin's hæmatokrit.

It is so constructed that one revolution of the large wheel, to which is attached the handle, causes the brass frame at the top of an upright piece of steel to revolve 104 times. This frame is so arranged that it receives two glass tubes, which are to contain the blood, each measuring thirty-five millimetres long, and held securely in position by a spring. The apparatus is extremely simple, compact, durable, and is admirably adapted for the purpose for

which it is intended. The glass tubes, that are to contain the blood to be rotated, have a capacity of 27·489875 cubic millimetres, are 3·75 millimetres thick, 35 millimetres long, and have a calibre throughout measuring one millimetre in diameter. On the outside is a scale dividing it into fifty equal parts, in the same manner as the scale on an ordinary thermometer. The method of determining the

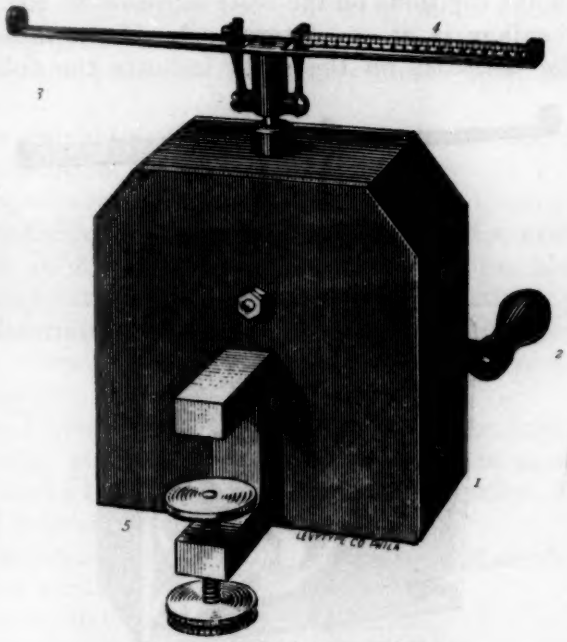


FIG. 2.—Author's hæmatokrit.

1, Box containing mechanism for rotating.
2, Handle of crank.
3, Frame which rotates 10,000 times per minute.
4, Improved capillary tube to contain the blood with scale.
5, Clamp to fasten apparatus to a table.

volume of corpuscles is very simple: the blood is mixed with an equal quantity of a fluid preventing coagulation, and is then rotated; the red corpuscles form a column at the periphery of the tube, and measure, we will say, twelve and one-half. As the blood is diluted one-half, this result is multiplied by 2 = 25, and to convert this into percentage

it is again multiplied by 2, as the scale is divided into but fifty parts, which gives fifty per cent.

The improved hæmatokrit (see *Figs. 2* and *3*), which I have suggested, and which I exhibit this evening, presents the following advantages:

The tubes (see *Fig. 5*) measure seventy millimetres in length, the lumen is *reduced* to one-half millimetre in diameter, and the divisions on the scale *increased* to 200, so that the percentage is at *once* determined. Under these conditions, the divisions on the scale indicate the volumetric

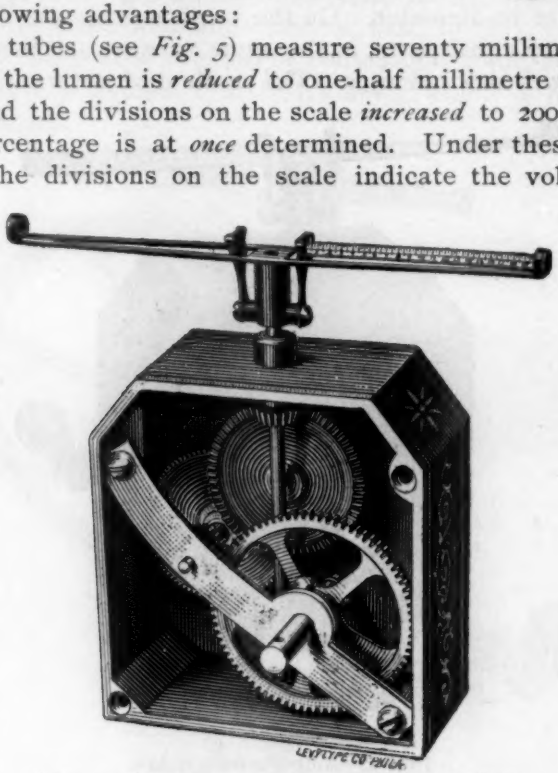


FIG. 3.—Interior view of author's hæmatokrit.

percentage. The longer column of blood will not only secure greater accuracy in reading the results, but also enables one to determine, with almost equal certainty, the volume of white corpuscles which, owing to their relatively lower specific gravity, form a second white column on the inner side of the red corpuscles.

The selection of a diluting liquid required much thought, time and labor. It becomes at once evident that this fluid should possess the following properties:

(1) It should absolutely prevent coagulation.

(2) It should preserve the normal shape of the corpuscles.

(3) It should harden the corpuscles so that no rupture occurs while rotating.

(4) It should give a *constant* volume in the *shortest* time.

(5) It should give the *largest* volume, so insuring greater accuracy in reading the result.

(6) It should possess a contrast color to that of the glass pipette, so facilitating and rendering more accurate the measuring of the diluting liquid.

(7) It should have no deleterious effects upon the white corpuscles.

(8) It should be of simple composition and form a permanent solution.

In experimenting with different liquids, blood from the same individual was taken, usually at the same hour, between 10 A.M. and noon, and afterward the blood that had been rotated was examined microscopically.

In determining the time required to obtain a fixed volume, observations were made every thirty seconds for ten minutes.

After a series of experiments lasting nine days, during which time nineteen different solutions were examined, I proved that a two and one half per cent. solution of bichromate of potassium fulfilled these conditions.

The solution possesses all the attributes of an ideal diluting fluid, namely:

(*a*) Absolutely preventing coagulation.

(*b*) Preserving the normal shape of the corpuscles.

(*c*) Hardening and so preventing rupture of the corpuscles.

(*d*) Giving a constant volume in sixty-six to seventy seconds.

(*e*) Giving the largest volume, thus diminishing the error in reading from the scale.

(*f*) Possessing a dark yellow color, contrasting strongly with that of the glass pipette, thus enabling one to measure the liquid with accuracy and ease.

(*g*) Staining the red cells so that the division between the column of red and white corpuscles is plainly seen.

(*h*) Apparently hardening the leucocytes.

(*i*) Making an extremely simple and stable solution, only requiring to be well corked to protect it from evaporation.

Lastly, an experience of four months, not only of myself, but also of my colleagues in the laboratory, *proves it to be by far the most useful liquid to dilute the blood for counting red blood corpuscles.*

A careful study was then made to determine the minimum amount of time required to secure a constant volume of red blood corpuscles, and after thirty observations it was shown that 100 revolutions of the larger wheel, or 10,000 revolutions of the frame carrying the tubes containing the blood, were amply sufficient when the 2·5 per cent. solution of potassium bichromate was employed. When the larger wheel was rotated 100 times rapidly, it required sixty-six to seventy seconds. Further rotation for one, two or more minutes was rarely successful in producing a reduction of one-fourth of a volume. In cases of extreme *leucocytosis* 200 rotations were found necessary, probably due to the relatively very light specific gravity of the white corpuscles.

In making a volumetric examination of the blood, the following articles are necessary:

(1) Alcohol.
(2) Ether.
(3) Large pin or a small lancet.
(4) Capillary pipette drawn to a point with a rubber tube.
(5) 2·5 per cent. solution of bichromate of potassium.
(6) Small watch-glass.
(7) Rubber tube to be attached to the glass tube of the hæmatokrit.

The lancet is first cleaned and then disinfected by the use of alcohol. The thumb of the left hand is most convenient. It should be thoroughly washed with ether and then allowed to dry, care being taken that no filaments from the towel remain. After making the incision, gentle pressure, causing the wound to gap, was usually found sufficient to produce a drop of blood the size of a pea; this was drawn into the pipette by suction, and an equal quantity of the bichromate solution added and thoroughly mixed in a

watch-glass. The hæmatokrit tube was then *immediately* filled by suction, one finger being held over the free end to prevent the displacement of blood, which would otherwise occur upon removal of the rubber tube. The filled tube is then placed in the frame of the hæmatokrit, and also a second prepared exactly in the same manner. The larger wheel is then rapidly rotated 100 times, and the results read from the scale, multiplied by 4, give the pecentage volume. The whole procedure should be done as *quickly* as possible, and particular care should be taken, as soon as the blood is mixed, that it be immediately rotated, otherwise the results obtained will indicate a larger volume of blood than really exists. The increased volume noticed upon allowing the mixed blood to remain in the watch-glass for a few minutes is probably due to the settling of the red blood corpuscles to the bottom because of their comparatively high specific gravity, and also to evaporation.

Having determined upon the method of pursuing the examination of blood by the hæmatokrit, it became important to determine the normal volume and its variations. For this purpose *thirty* healthy medical students, with an average age of twenty-four, were examined, and the percentage volume varied from sixty-two to forty-four, averaging fifty-one per cent.; a similar observation upon eight female nurses showed a variation of from forty-two to thirty-six, averaging forty-four per cent., or seven per cent. less than in the males. As the blood-counting apparatus is considered the best method of determining the number of red and white blood corpuscles, an attempt was made to show their relative accuracy, and also the probable number of corpuscles for each percentage volume. To accomplish this the blood from twenty-five healthy men, physicians and students, was examined for the percentage volume, and at the same time a count was made with the following result: The average age was twenty-six years; the maximum volume observed was sixty-six per cent., and the minimum forty-four per cent., and an average of all the cases examined gave 51·8 per cent. volume. The red blood corpuscles in each case were carefully counted, and for this purpose the Thoma-

Zeiss hæmacytometer was employed. The average of all the counts made gives 5,088,442, and this being the case, one per cent. of volume is the equivalent of 98,578 red blood corpuscles. The difference between 99,390 and 100,000 is but 610; and, as in disease the average volume is reduced to 33·5, the greatest possible difference is but 20,435, a number so small as to be safely considered of no practical importance. In my opinion, therefore, for convenience, *one percentage volume may be considered as representing 100,000 red corpuscles.* This number greatly facilitates the calculating of the number of corpuscles present. When the percentage volume has been obtained, all that is necessary is to add five ciphers, and the number of corpuscles are indicated.

It is extremely interesting and important to note that the average volume of the thirty cases examined gave 51·5 per cent., and that this series of twenty-five gave 51·8 per cent., a difference of ·3 per cent., or 29,361 red blood corpuscles. These two series of observations, numbering in all fifty-five cases, prove most conclusively, first, that the average physiological volume for young men about twenty-six years of age is 51·6 per cent., and further, the fact that the average volume in both series speaks well for the *accuracy* of this method. With reasonable care the error by this method should fall within four volumes, or, roughly speaking, should not exceed 400,000; and when the modified apparatus here shown is adopted, it is quite certain that this working error may be reduced fifty per cent. or more, so that one may confidently expect that the volumetric reading shall fall within 200,000 of the true number of corpuscles, a deviation from normal so slight as to be of no practical importance.

From my own personal experience, and that gathered from my colleagues regarding the results obtained by the use of the Thoma-Zeiss hæmacytometer, a difference of two-, three-, five-hundred thousand, or more, is not at all unusual.

Reinert has clearly shown that this instrument gives a *theoretic* error of three per cent., or 150,000, in the hands of experts, and in *practical* clinical work, as will be proved later, it is certain that this error is considerably increased.

In making these observations, an effort was made to determine the normal volume of white corpuscles, but the extremely narrow white band, scarcely a line in width, renders accurate observations extremely difficult and often impossible. It was noticed, however, that in health the column of white corpuscles was never wider than one of the division lines cut in the glass tube. Any *considerable* increase in the white corpuscles is at once detected. The glass tube is modified, as here shown (see *Figs. 4* and *5*), so that the diameter of the lumen is decreased and the length doubled, permits of a more satisfactory study of the white corpuscles.

An analysis of the series of twenty-five cases referred to shows that the average age was twenty-six years; that the maximum volume observed was sixty-six per cent., and the

FIG. 4.—Original capillary tube, exact size.

FIG. 5.—Author's modified tube, showing its *exact* size and precise appearance of the scale.

minimum forty-four per cent., and an average of all the cases examined gave 51·8 per cent. volume. The red blood corpuscles in each case were carefully counted, and for this purpose the Thoma-Zeiss hæmacytometer was employed. The average of all the counts made gives 5,088,442, and this being the case, one per cent. of volume is the equivalent of 98,578 red blood corpuscles.

With this improved hæmatokrit one may make an examination in from five to ten minutes and determine at a *glance* the *volume* and *number* of *red* and *white* corpuscles.

In many of my cases the volume and number of corpuscles do not agree when five ciphers are added to the percentage volume, and this may be explained:

(1) By the variable results obtained, especially when we make but one preparation, and count sixty-four squares, as was done in the majority of the cases reported, and

(2) That the blood used to determine the volume was always mixed with the bichromate of potassium solution in the ampulla of the Zeiss pipette, and not separately measured and mixed in a watch-glass; and therefore, in all cases where the tendency to coagulation was increased, the volume was eight or more per cent. less than really existed.

From the studies which I have here recorded, the following opinion is deduced *that the hæmatokrit gives as accurate, if not more accurate, results than the Thoma-Zeiss apparatus as ordinarily employed, requires less skill, calls for no eye-strain, and the volume of red blood corpuscles, and number per cubic millimetre, and volume of white corpuscles, may be determined within ten minutes.*

In conclusion, I beg leave to call your attention to a table, showing variation in the volume of the blood cells in a variety of diseases.

A NEW TELE-PHOTO LENS.

By Julius F. Sachse

[*Read at the stated meeting of the Institute, held May 17, 1893*]

Mr. President and Members of the Franklin Institute, of Philadelphia:

Towards the close of the year 1891 the scientific world was startled by an announcement made by Dr. Adolph Miethe, of Berlin, before a meeting of the Society of Practical Photographers, in Berlin, that a photographic objective had been constructed upon an entire new principle, which would overcome distance to an extraordinary degree, and with a normal extension of camera length would produce results heretofore thought to be impossible.

At this meeting, held November 4, 1891, a specimen print was shown, the image of a monument on the Rhine taken from the opposite shore at a distance of over 2,000 metres, and measuring ten centimetres.

Almost simultaneously with this announcement came one from the noted English optician, Thomas R. Dallmeyer,

SHOWING THE VOLUME AND NUMBER OF RED AND WHITE CORPUSCLES.*

Number of Case.	Age.	Sex.	Volume of White Corpuscles.	Volume of Red Corpuscles.	Number of Revolutions.	Hæmaglobin Fleischl Test.	Number of White Corpuscles.	Number of Red Corpuscles.	Probable Number of Red Corpuscles as Estimated from the Volume.	Diagnosis and Remarks.
						per cent.				
1	27	F.	1	42	10,000	5'4 or 38	6,783	3,220,000	4,200,000	Diabetes with typhoid fever.
2	34	M.	1	30	10,000	7'4 or 53	10,150	3,150,000	3,000,000	Hepatic cirrhosis. Internal jaundice Blood looked red and watery.
3	20	M.	1	40	10,000	11'2 or 80	6,433	5,950,000	4,000,000	Acute croupous pneumonia. Third stage.
4	20	M.	1	44	10,000		7,155	5,775,000	4,400,000	Same case two days later.
5	21	M.	2	54	20,000	11'9 or 85	19,803	5,975,000	5,400,000	Acute croupous pneumonia. Second stage.
6	21	M.	2	48	20,000	—	13,630	5,463,000	4,800,000	Same case two days later.
7	33	F.	1	12	10,000	—	6,210	1,450,000	1,200,000	Gastric carcinoma. Intense anæmia. Blood looked pink.
8	65	M.	½	12	10,000	1'4 or 10	3,203	600,260	1,200,000	Essential anæmia. Blood pink and watery, and bled freely from puncture.
9	65	M.	½	8	10,000	10	3,900	562,500	800,000	*Ibid.* Diagnosis confirmed by autopsy.
10	43	M.	4	40	20,000	—	28,523	4,962,500	4,000,000	Acute croupous pneumonia. Second stage.
11	43	M.	1	44	10,000	80	9,050	5,437,500	4,400,000	*Ibid.* Convalescent and afebrile.
12	17	M.	2	32	10,000	—	15,414	5,375,000	3,200,000	Acute rheumatic pericarditis.
13	17	M.	2	36	10,000	—	18,471	4,720,000	3,600,000	*Ibid.*
14	17	M.	2	36	10,000	7'7 or 55	18,662	4,412,000	3,600,000	*Ibid.*
15	19	F.	1	30	10,000	7'7 or 55	8,340	4,180,000	3,000,000	Chronic endocarditis. Acute pleurisy. Temperature 38° C.
16	58	M.	1	28	10,000	—	8,057	3,181,808	2,800,000	Gastric carcinoma.
17	46	M.	1	24	10,000	—	7,070	3,593,750	2,400,000	Chronic peritonitis with ascites.
18	33	M.	1	20	10,000	—	12,102	2,230,000	2,000,000	Intestinal hemorrhage.
19	28	F.	3	50	20,000	—	24,000	5,250,000	5,000,000	Fourth day of acute croupous pneumonia of right lower lobe.
20	28	F.	3	48	10,000	—	22,484	5,787,500	4,800,000	*Ibid.* Eleventh day of the disease. No fever.
21	70	F.	2	18	10,000	35	32,000	2,712,500	1,800,000	Chronic interstitial nephritis confirmed by autopsy.
22	46	M.	4	28	20,000	45	32,248	3,637,500	2,800,000	Malignant sarcomatosis confirmed by autopsy.
23	15	M.	1	17	20,000	30	1,719	2,062,800	1,700,000	Lymphatic and splenic pseudo-leukæmia.
24	15	M.	½	16½	10,000	—	2,102	2,160,000	1,650,000	*Ibid.*
25	15	M.	½	16	10,000	30	4,012	2,187,500	1,600,000	*Ibid.*
26	21	F.	1	42	10,000	s —	11,783	5,962,500	4,200,000	Acute Bright's disease.
27	38	F.	1	33	10,000	s —	9,936	4,410,000	3,300,000	Pregnancy.
28	45	M.	1	36	10,000	s 55	3,503	4,956,000	3,600,000	Tertian intermittent fever.
29	36	F.	2	36	10,000	s —	17,006	5,237,000	3,600,000	Acute myelitis.
30	29	F.	13	20	20,000	s 40	330,956	3,025,000	2,000,000	Splenic leukæmia.
31	29	F.	13	24	20,000	s 40	275,000	2,900,000	2,400,000	*Ibid.*
32	29	F.	13	25	20,000	s 40	181,250	3,160,000	2,500,000	*Ibid.*
33	29	F.	13	22	20,000	s —	180,000	3,575,000	2,200,000	*Ibid.*
34	29	F.	1	33	10,000	s —	6,496	3,470,969	3,300,000	Tumor cerebri syphilitic.
35	20	M.	2	36	20,000	s 65	16,123	4,830,000	3,600,000	Acute articular rheumatism.
36	20	M.	2	42	20,000	s —	7,126	4,830,000	4,200,000	*Ibid.* Articular rheumatism.
37	33	M.	2	48	20,000	s 12'6 or 90	13,695	5,864,000	4,800,000	Perityphlitis, afebrile.
38	21	M.	1	50	10,000	—	—	5,037,500	5,000,000	Chlorosis.
39	21	M.	1	52	10,000	55	—	5,387,500	5,200,000	*Ibid.*
40	67	M.	1	30	10,000	s 65	7,197	3,974,500	3,000,000	
41	23	F.	1	27	10,000	35	6,170	3,200,000	2,700,000	Chronic parenchymatous nephritis.
42	27	F	1	38	10,000	—	4,108	4,637,500	3,800,000	Simple anæmia.
43	24	M.	1	38	10,000	—	—	3,937,000	3,800,000	Simple anæmia.
44	40	M.	2	70	10,000	102	12,172	6,912,000	7,000,000	Cholera nostra.

* The counts here given made by my colleague, Dr. Carl Seller, are marked "s."

giving notice of a similar invention by which he had obtained, with an ordinary camera, the image of a crow in mid-air over 100 yards distant, measuring about three-fourths of an inch from tip to tip.

The description of the new objective of Dr. Adolph Miethe, together with specimen prints were first brought before the American public by the speaker in the February and March numbers of the *American Journal of Photography*, for 1892. Subsequently the matter was brought to the notice of this Institute by one of your members.

These specimen results, which showed one of the greatest advances made for years in photographic optics, naturally created considerable attention in scientific quarters, and numerous experiments were made to construct similar lenses in this country.

One of the most enthusiastic experimenters in this field of photographic optics was your fellow-member, the late W. A. Cheyney, a man thoroughly versed in the science of optics. Mr. Cheyney's experiments were conducted upon similar lines to the foreign ones, as laid down by Dr. Adolph Miethe, viz: A combination of the regular objective with a negative element fixed in an adjustable draw tube, independent of the adjustment of the camera-bellows. Specimens of results obtained by him showed satisfactory progress; his labors, however, as you know, were interrupted by his untimely death.

Another experimenter, who became interested in the new departure in photographic optics, was Mr. Albert B. Parvin, also of this city, and I now have the honor to introduce to you for your inspection and consideration a result of his labors, viz: A new tele-photo lens, which differs in construction and action from its European prototypes.

In the first place, the new objective is contained within an ordinary lens tube two and five-eighths inches long, and it requires no delicate adjustment prior to taking the camera focus. Secondly, the remarkable rapidity or speed with which the new lens works, in direct contrast to the European systems which are not rapid enough for instantaneous work. A specimen print which I have here to show you, according

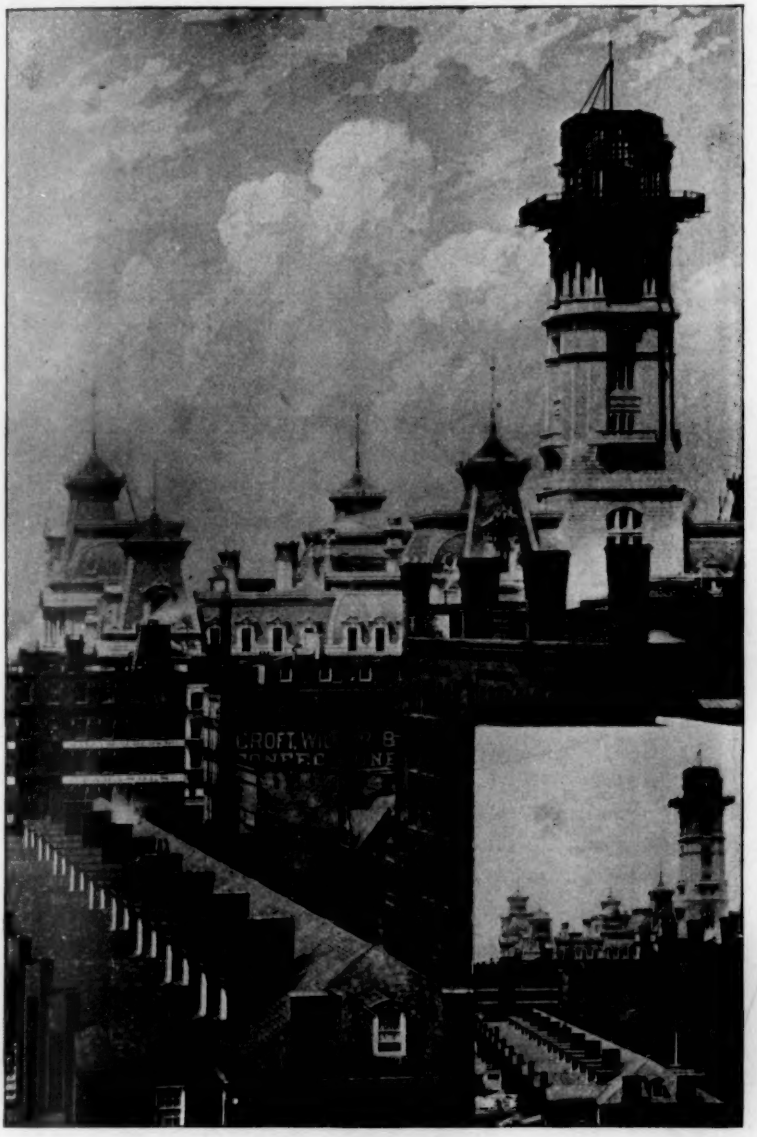
to the maker, was taken on a cloudy day, at half a mile range, stop f/42, in one-fiftieth of a second. The picture certainly shows excellent illumination and sharp definition, even the cordage of the vessel being clearly cut. Another pecularity claimed for this new tele-photo lens is its covering power. The lens here for your inspection is one and three-thirty-seconds of an inch in diameter, equal to what is known as a Ross 6 x 5 lens, yet, as you will perceive by the specimens here shown, it more than covers a 10 x 12 plate sharply, while the same test (*C*), made with a 5 x 8 Beck lens, which is about an inch and a quarter in diameter, falls far short of covering the plate.

As to the "tele-focal" powers of the new lens (if I may use the term), a comparison of the two views, marked *A* and *B*, will give some idea of its scope. Both are taken from the same standpoint, the former (*A*) is taken with the new Parvin lens, the latter (*B*) with a Beck lens of almost the same diameter. These three experiments *A B* were all made by the inventor from the same standpoint. Exposure, *A* two seconds, *B* three seconds, *C* two seconds, stop in all *f*/64.

An examination of the Parvin picture will show the depth of focus and its claim to absolute rectilinearity.

As to my personal experience with the new tele-photo objective, I will state that upon two occasions I had an opportunity to test the new objective, both experiments were, however, confined exclusively to trials in focussing upon the ground glass, the main object being to calculate the magnification, and determine its powers and possibilities in actual practice and its adaptation for scientific work, such as falls to the every-day lot of the naturalists or geologists, by affording means to record photographically objects which are beyond the focal length of the ordinary objective. These comparative tests, made with both 8 x 5 Darlot objectives, showed that the magnification of the new lens was about five times greater than the regular objectives of equal diameter.

Excellent results were shown upon formations distant by actual stepping, from 100 to 1,000 yards, the finest details being sharp and clear-cut with an open aperture, the camera extension being fifteen inches.



Comparative test of the Parvin tele-photo lens and rapid rectilinear lens of equal diameter, both taken from the same standpoint.

In focussing upon distant objects, distant from three to four miles, such as the Fairmount Observatory from Belmont, the iron rods and structure showed plain and distinct against the sky.

The same may be said of the tower of the new City Hall, distant four miles from the same standpoint, which with the old objectives from the same standpoint are hardly discernible. These results led me to believe that tele-photo lenses of this character, *i. e.*, of a system of moderate amplification, would be of considerable value in the hands of the scientific student and photographic tourist. Such lenses, as they become better known, will no doubt prove indispensable to the amateur and tourist, as it will give opportunities for obtaining views and bits of detail otherwise unobtainable with the means which under ordinary circumstances are within the reach of the travelling amateur. Such lenses offer to him magnification in addition to rapid work and sharp definition. For very close work, such as laboratory or studio work, I find that the new lens is not so well adapted as some of the older forms of objectives, the magnification being a fixed one. This, however, is not to be taken as detrimental to the new objective, as the sample furnished for experiment was intended entirely for distant outside work. It is stated, that in the near future, specimens will be forthcoming where the same principle is adapted for interior work.

In conclusion, I will state that no such extreme telescopic properties are claimed for the new Parvin lens as have been shown by the German and English lenses. I allude to such views as that of Mont Blanc, taken from Belleview near Geneva, at a distance of forty-four miles, by Fred Boissonas, which was lately exhibited in this city. Further, this view was made in a camera built expressly for the purpose, with a draw of sixty-six inches, the time of exposure being ten minutes.

The claim made for this Philadelphia tele-photo lens is that it is a simple and practical objective, having a fixed system of magnification, the whole being constructed for ordinary use in the hands of the average photographer of the day.

One of the greatest advantages claimed for the new lens, over all the European telescopic lens system, is the fact that the new lens works instantaneously even when stopped down to f/45. The *Photographische Nachrichten*, published in Berlin no later than April 27th last (p. 216), distinctly mentions that the German tele-photo objectives are not suited for instantaneous work. Here we have proof of the claim of the new American objective. I submit for your inspection a number of comparative photographs, all made by the inventor of the new lens, which will enable you to judge intelligently of the magnification, covering power rectilinearity and speed, some of them being produced by an exposure of one-fiftieth of a second.

I now leave the subject in your hand for further investigation.

THE MANUFACTURE OF ANTIQUE PERSIAN RUGS.

BY H. ENDEMANN, Ph.D., Chemist.

Some time ago I found in one of our daily papers an article complaining of the sale of imitation antique rugs for the genuine article. Having come into the possession of one of these, I experimented with it, not only with the view of devising a plan for readily detecting the spurious nature of these goods but, incidentally, regarding the probable method by which they were produced.

Later on I will give a method which is capable of producing as beautiful a specimen of an antique rug from a bright-colored Persian rug as ever delighted the eye of antique hunting buyers. The rugs as found, however, show all more or less traces of their treatment. On close inspection it is easily observed that some of the bright colors of the wool have run into the cotton margin, which is itself discolored throughout and gives reactions for iron as well as tannin compounds, but otherwise the rugs look as if really age only had wrought the changes observable.

The wool is more decolorized on the outside than on the inside, where it is firmly held by the warp, and an inspec-

tion in this direction will therefore not reveal the spurious nature of the goods. For this fact of a more thorough destruction of the color on the surface where atmospheric influences, as moisture, air and light, have acted more thoroughly is true likewise for the real antique article, where we find the color destroyed also more on the surface than on the inside.

Also the softness of the artificially made antique rug compares well with that observable in the genuine goods.

An examination of the colors on the wool is hopeless when we have to deal with colors destroyed and changed to such an extent as I found them in this class of goods. The real test can therefore be best made on the cotton margin by chemical reactions and general inspection.

The preliminary examination taken into account, I set out to unearth the probable mode of manufacture.

It was soon found, that all bleaching agents, while they removed sufficient color in some cases, left the colors rather in bright shades, and alone could therefore not have been used for the production of the antique rug, *i. e.*, the colors must be toned or shaded to produce the desired result.

I shall not go into the details of all possible methods but go at once to the description of the method which I consider to be the most serviceable and therefore very likely the one used in the production of such spurious goods as I had occasion to see. The first step is to discharge most of the color by means of an alkaline bath.

For ten rugs (25 x 40 inches each) dissolve one pound of borax in 120 gallons of water and heat to about 140° F., then prepare a concentrated solution of caustic soda, two and one-half pounds to about two gallons of water.

Then add one-third of this solution to the warm borax solution and enter your rugs, working them constantly by hand or reel, at the temperature mentioned. During one hour while they are worked the rest of the caustic soda is added in small portions, taking the precaution that the newly-added alkali is thoroughly mixed with the old solution before the rugs re-enter.

At the end of this time considerable color will have

passed into the solution, which is then run off and replaced by fresh water repeatedly to remove the alkali.

The rugs are then allowed to dry, when dry they are put for a short time into a solution of extract of fustic, one to 100, and again allowed to dry (one pound extract of fustic to twelve gallons of water). They are now bleached in the following solution: One gallon commercial hydrogen peroxide is mixed with thirty-five gallons of water and thereto is added one-fourth gallon strongest aqua ammonia.

The rugs are entered and remain therein until sufficiently decolorized. The colors now, though quite towards the yellow, are still bright.

The rugs are now taken out, wrung and at once brought into a bath of four fluidounces of tersulphate of iron (*United States Pharmacopœia*) in about fifteen gallons of water, where they remain until the shade has become sufficiently dirty to suit the operator. Slightly changing the conditions, as, for instance, not allowing the rugs to dry, when mentioned in the process, or changing the strength of the solution and the time of exposure will give corresponding results in producing different shades, which may account for the difference of shades of such spurious goods as found in the market.

It is perhaps needless to say, that the cotton margin gives identical reactions with those observed in spurious goods found in the market. Treatment with a neutral solution of ferric chloride produces a greenish-black color. The iron can be seen by the rusty color of the ashes and additional tests if required. I do not anticipate that rugs so treated will show the same keeping qualities as the real antique rug. The fact that the colors in the rugs have not been changed but bodily taken out will certainly result in the almost complete destruction of the small quantity of color left, especially under the oxidizing influence of the iron salt introduced for the purpose of toning.

NEW YORK, July 17, 1893.

THE UTILIZATION OF GARBAGE.

BY DR. BRUNO TERNE.

[*A paper read at the meeting of the Chemical Section, held June 20, 1893.*]

The immortal Justus Liebig, the founder of agricultural chemistry, and, indirectly, by his teachings, the creator of the industry of artificial fertilizers, combined in himself the qualifications of a most eminent chemist and a far-sighted national economist.

He transformed the chemical doctrines of plant nutrition at once to available figures for the farmer to show him in clear and indubitable sentences the laws of nature which compel him to restore to the soil what he has taken from it in the form of the products of the field.

His classical *Chemical Letters* are a monument he has erected for himself which will endure as long as the conditions of this globe remain the same as they now are. As long as the human race lives, the fundamental laws requiring the restitution to the soil in available form of the constituents of the plants as laid down by Germany's most eminent agricultural chemist will be written in golden letters in the history of all civilized nations.

In centuries to come the classical works of Liebig will be revered as the revelation of science respecting the practice of agriculture.

In his forty-seventh letter, Liebig refers especially to the relation of the consumption of the great cities to the products of our farms. From each hectare of wheatfield, the farmer, from the return of an average harvest transports from his farm to the consumers, 4,000 pounds of grain containing seventy pounds of mineral substances, mainly phosphoric acid and potash.

The refuse of a city of 1,000,000 inhabitants will amount, in dried form, per annum, to 45,000,000 pounds.

The constituents of this powdered material are 10,300,000 pounds of mineral substances, mostly the mineral parts of

breadstuffs and meat, which contain no less than 4,580,000 of phosphatic salts.

The removal of this precious material (at the rate above-named) from the fields has been going on for centuries and only a small portion of it finds its way back to the fields.

It is folly to think that the loss of this material has had no detrimental influence upon the fertility of the soil.

I will quote from the tenth letter of Henry C. Carey, the American statesman and national economist, to the President of the United States. [J. B. Lippincott, Phila., 1858.]

"The national economical question is not how much we can produce but how much is returned to the soil of the products annually.

"Labor used to rob the soil is worse than labor thrown away. In the latter it is a loss to the present generation, in the former poverty for the coming generation."

The same system which Carey condemned, in the above citation, of a large export of material without corresponding restitution of plant food to the soil continues to be practised in the corn belt of the West, while the cotton belt of the South and the entire East have been compelled to yield to the inexorable law of nature, and must now restore food to the impoverished soil to raise their crops.

The zone subservient to artificial nutrification of the soil grows wider and wider from year to year, since the waste of plant food-material grows more serious as our population grows denser.

What Carey so incisively pointed out thirty-five years ago, is as true to-day as it was then, but only the stress of absolute necessity, or the prospect of financial gain, will suffice to alter the system involving the extravagant waste of plant food-material which is now the rule with us.

For the sake of convenience we pollute our rivers and choose rather to suffer the consequences of drinking polluted water than to adopt rational measures to save, for the enrichment of our fields, the products which the law of nature has provided for the very purpose.

Our antipodeans, the much-despised Chinese, are much better practical economists than their civilized brethren

In China, nothing is allowed to go to waste that is useful to the soil. The Chinaman's house is without the improved devices of the modern plumber, and his scent for nuisances may not be so highly cultivated as ours, but by his system of the strictest economy, he manages to keep up the fertility of his soil to so high a degree, that the vast empire, notwithstanding that it possesses the densest population on the face of the earth, is entirely independent of all other nations, not only for the breadstuffs and other food supplies required for the sustenance of its people, but also for the fertilizing materials needed for its soil.

The Chinese, with the instinct of self-preservation, have been doing for centuries what Liebig and his followers taught the civilized nations of the world in the beginning and middle of the present century.

To-day we have fleets engaged in transporting phosphoretic and nitrogeneous materials from continent to continent; the services of the miner and the skill of the chemist are required to supply this food-material to our exhausted fields.

The nitrate mines of Chile, the phosphate beds of South Carolina and Florida, of Belgium and Russia, the mountains of apatite rock of Estremadura and the deposits of the same material in Canada, the potash salts of Germany and Hungary, are drawn into the service of agriculture. One of the most gigantic branches of the chemical industry has been built up in order to balance the debit and credit page of the rational farmer in the records of his culture.

But in seeking for remedies that will preserve the fertility of our fields, it is a remarkable fact that a vast amount of waste material which lies right at our feet, is not given a thought.

It is true that all the more valuable waste materials, such as bones, tankage, cracklings, cottonseed meal, leather, hoofs, horns, etc., have a stable market. But the poorer and much more abundant materials have been, and are now, almost entirely neglected.

We will to-night direct our attention to the question of the possibility of utilizing the garbage of our city, which, at the present writing, is the subject of much discussion.

Until recently the garbage collected in this city has been used mainly as a feeding material for the fattening of pigs in the outskirts. For sanitary reasons, the Board of Health has banished the piggeries outside the limits of the county of Philadelphia. This is very commendable to free the city of a nuisance, which at the best was a very crude method of disposing of the material, and which, by the carelessness of those who make a business of it, certainly created a health-endangering nuisance; but the decision of the Board of Health made the disposal of the material very much more difficult than before.

The city of Philadelphia is divided into five districts for the collection of garbage. During the summer months, from June to September, the accumulation of a single district runs up to 100 tons per day, while in the other months of the year the daily output varies, and in the middle of the winter falls to twenty-five tons per day.

A general average for the whole year is about 250 tons per day for the whole city.

This estimate we can safely use for calculation of returns but not as a basis for the construction of a plant, for which the maximum figures will have to be taken.

For the first district, comprising all that part of the city south from South Street, from river to river, a furnace for the cremation of garbage has been erected, situated on Washington Avenue and Twentieth Street.

Unquestionably cremation is the most complete system for destroying all organic substances, and doubtless to the extreme sanitarian the only method that should be adopted.

But what about the economical results? The daily operation of the furnace requires labor and fuel, the product of a cremator is a small quantity of ashes and worthless at that. Four or five per cent. of ashes is all that remains of the garbage when incinerated.

The American Incinerating Company, with headquarters at Washington Avenue and Twentieth Street, has offered its product to the fertilizer trade. I have here a sample of these ashes which were sent to us. Our analysis yielded us:

Total phosphoric acid = 15·32 per cent. (= 33·88 per

cent. phosphate of lime), potash 0·25 per cent., soluble in water.

I was astonished to find so little potash but the intense heat in the furnace has driven off a part of the alkalies as chloride vapors, otherwise the analysis should show at least four times as much.

This ash, even with fifteen per cent. phosphoric acid, has a very low market value because natural phosphates in bags, and ground to the finest powder, can be laid down at our doors at $5 per ton.

The phosphoric acid of such low percentage is valueless for dissolving purposes because it will only yield practically a powder containing five to six per cent. of available phosphoric acid. This low product cannot be used for mixing fertilizers and it cannot be offered to the market when acid rocks containing thirteen to fourteen per cent. of available phosphoric acid are offered as low as $10 to $12 per ton.

The products of the cremating process, therefore, will never become a commercial article. The question then arises, Can the same sanitary requirements be reached in another way? Without doubt they can, and, at the same time, the valuable constituents of the garbage destroyed in the furnace can be saved.

The city garbage, embracing all the refuse from the table and kitchen, changes its qualities considerably with the season. Just now we are in the vegetable period and the results of my experiments represent about the lowest results obtainable.

The experiments I have made are not laboratory experiments, but practical manufacturing experiments, handling at a time from six to twelve tons of material. I have been in the fortunate situation of being able to utilize, at intervals, apparatus designed for other purposes but very well adapted for the purpose of these experiments.

There is no secrecy about the principle of rationally utilizing garbage, and there are no new processes involved for which enormous patent fees will have to be paid.

The process is divided in two main operations:

(1) The separation of the grease by extraction.

(2) The drying of the remainder to form directly a salable product.

The results I have obtained yielded me about seventeen per cent., on an average, of dry product from garbage tankage, a sample of which you have before you. This dry tankage is a very excellent fertilizing material. Its composition is:

	Per Cent.
Moisture,	4·41
Organic matter, (including 4·3 per cent. NH_3)	73·34
Mineral matter,	22·25
	100 00

On a smaller scale than that which could profitably be carried on in Philadelphia, garbage is utilized successfully in this way in several places. I have received from Detroit samples of fertilizer made from garbage with as high as 5·37 per cent. ammonia, 6·08 phosphoric acid; and, at another time, 3·76 per cent. ammonia, 3·36 phosphoric acid.

A specimen from Providence contained 3·55 ammonia, 3·38 phosphoric acid; from other places, 3·86 ammonia, 3·51 phosphoric acid.

A fair average analysis, showing the minimum and maximum of what we may export, is as follows:

	Per Cent.	*Per Cent.*
Ammonia,	3 50	4·50
Phosphoric acid, (corresponding to phosphate of lime, 6·54, 13·28.)	3·	6·
Water soluble potash,	0·25	0·50

At the present market price the unit of ammonia is sold at 2·60. This would give, for the low grade, a market value of $9.10 per ton; for the high grade, $11.10 per ton.

It is safe to say that at the present market these products will realize $10 per ton. I leave phosphoric acid and potash out of the calculation to give you the practical valuation.

This material is a most excellent fertilizer, not only chemically but also physically, and its usefulness in the manufacture of fertilizers is without limit.

The amount of garbage tankage produced from a ton of material as gathered up from the houses, placed at the low-

est percentage (viz: about fifteen per cent.), amounts to 300 pounds per ton. One hundred tons per day will give 30,000 pounds or fifteen tons daily.

Taking the daily average of one district, we will average seven and one-half tons, or 2,250 tons per year; and for the five districts, 11,250 tons, having a market value of $112,500.

Now, let us see what we restore to the fields in this amount of products.

The ammonia taken at four per cent. in one ton of the tankage contains eighty pounds of this important plant food, and 112,500 tons, or the amount of production from the garbage of this city should be equivalent to a saving of 9,000,000 pounds of ammonia, 9,000,000 pounds of phosphoric acid, and 500,000 pounds of potash.

All of this has been taken from our fields, and, if wasted or destroyed, represents so much loss to the community at large, but if this amount be regained and restored in proper form to the farm and garden it means so much saving. All of this amount must be restored from other sources in some way. If any waste material is worth saving it is surely the garbage of the cities, as I have endeavored to show.

There is another feature connected with this waste, viz: the regaining of the grease contained in garbage.

City garbage, at a low estimate, will yield three per cent. of a black grease, which you see in the samples before you. Even for this black grease, there exists a limited market at low figures for lubricating purposes (for car wheels). This black grease rarely contains much free fatty acid but it is by no means free from it. I have had samples containing over six per cent.

If the regular production should rise to such quantities as the garbage of the whole city will permit of producing, other uses must be found for this product, which, in the crude state has but a limited field of usefulness. There are ways and means known to the chemist, however, to improve this raw product, and the refined material made from it will readily find a market in competition with other greases.

The result of a chemical artifice of this kind, I take pleasure in introducing to your notice in the sample before

you. The method employed by me in this work is absolutely practical in respect of quick action, cheapness and positive results.

The grease stock so produced is very oily at 60° F. It yields about seventy to seventy-five per cent. of this very fine oil which will find a ready sale. This oil will prove a puzzle to the best expert in the analysis of oils, for it contains traces of all oils and greases which enter the kitchen. It will challenge the reliability of all the color tests, the iodine numbers, etc., but in spite of its evasion of the methods of a Mailliau and Benedikt, it will find an excellent market.

The refined grease is a very fine soap stock, and a very crude product of a test in that direction I have with me. This soap is made solely from the same grease you have before you.

The rational mode of disposing of the city garbage is as simple as anything can be.

First, to gain all the grease, we must apply practical methods of extraction by known solvents; then, to save the fertilizer materials, we must employ the most rapid and economical methods to expel the eighty per cent. of water contained in the material.

Small amounts are easily handled; the difficulty arises when such enormous quantities stare one in the face. It is not, therefore, so much the chemical processes employed as the proper disposition of the plant, which must be well understood.

The stock delivered at the works must disappear in the process, on its arrival, and must never be permitted to rest for a moment until, after forty-eight hours, it is ready to be filled into bags for shipment in the form you see before you in this sample. It is no more difficult to handle the enormous quantities of garbage than it is to handle the animal refuse of our city.

My experience of many years in handling materials of this kind in enormous quantities, first in Chicago, and since 1877 in this city, as the chemical manager of the largest works of its kind, permits me to speak advisedly on this

subject, and I am prepared to stake my reputation as a technical chemist on the assertion that the utilization of our city garbage can be carried on as a financially successful operation for the saving of the valuable materials contained therein. How important the solution of this question is for the city of Philadelphia is well illustrated by the constant discussion of the subject in the daily papers.

Cremation, as I have shown, produces, at considerable expense, a valueless product. The rational chemical process yields, at no more cost, products which make a large figure in the housekeeping of a community.

Should we cast aside the warnings of an economist like Carey? Shall the teachings of Liebig and of all the prominent agricultural chemists of all nations have been in vain? I trust not, and believe not.

In this century of progress, with our knowledge of chemistry, and with the most complete machinery at our disposal, it seems to me like a lapse into barbarism to destroy this most valuable material simply for the purpose of getting rid of it, while at the same time, we are eager to obtain these very same materials for our fields by purchase from other sources.

There is no doubt in my mind that this question of the disposal of our garbage can be solved by careful consideration of all points, by practical business men, to the advantage of the city authorities and the contractors for this work, to the fullest satisfaction of the health authorities, and to the benefit of the farmer.

Capital intelligently invested should be productive, not destructive. Instead of spending thousands of dollars for the erection of crematories to destroy, let us erect sanitary chemical works to preserve, this valuable material. There can be no danger to the public health in the conduct of a rational system for the utilization of garbage. All microbic carriers of contagious sicknesses are destroyed by a temperature of 212° F., and this dry product you have before you is as harmless to the public health as the flour in the barrel.

My desire and expectation to-night, in making this

presentation of an important industrial and sanitary problem, is to receive the endorsement of the Section, and later on, that of the Institute, to have you condemn the destruction of these valuable waste products, and approve, as the only right solution of this question, the preservation of these products displayed before you by a rational process of manufacture such as that indicated above.*

ARTESIAN WELLS.

By Oscar C. S. Carter.
Professor of Geology and Mineralogy, Central High School, Philadelphia.

[*Read at the stated meeting of the Chemical Section, held March 21, 1893.*]

Within the last few years a great many artesian wells have been drilled in Pennsylvania to obtain pure drinking water for towns, villages and farms, and also for various manufacturing and industrial purposes, such as the manufacture of artificial ice, the brewing of beer and for use in steam and locomotive boilers. The two principal questions to consider in artesian water are the quality and the quantity. First, as regards the quality, there can be no doubt that water which filters through from 200 to 500 feet of rocky strata is free from all deleterious organic matter. Disease germs, which sewage in river water renders so common, are unknown in artesian water unless the well be drilled in a thickly populated district or be improperly cased. Artesian water comes to the surface clear and sparkling and is

* The very able report on the above paper, which appeared in the *Public Ledger*, seems to have given to some persons interested in this question, the wrong impression, that it was presented in the interest of the company with which I am connected. Such is not the case.

This question, like any other question of interest to the company, has been fairly examined into by its chemical manager, but the company, for business reasons, would not undertake the utilization of garbage.

The chemical manager, therefore, was at liberty to make use of the results of his experiments for the benefit of the public at large, and did so after consultation with the president of the company and with his approbation.

B. T.

never muddy from clayey matter or dirt held in suspension. The geological formations of Southeastern Pennsylvania contain prolific water-bearing strata. Wells drilled in the sandstones, clay slates, and shales of the trias, in the mica schists and gneisses of the Philadelphia group, in the Silurian limestone and in the Potsdam sandstone, generally yield an abundance of water; occasionally, a barren locality is met with, but this is rarely the case. It would be impossible to lay down any hard-and-fast rule as to which formation furnishes the most abundant supply of water; an examination of the records of the accompanying wells will give some idea. The new red sandstone shales and slates and the mica schists of the Philadelphia group yield an abundant supply of water. The Silurian limestone in this vicinity often dips steeply, but gives a good supply. Thus far, wells drilled in the Potsdam sandstone have not yielded as much water as the above formations, although a good supply is obtained. It may be stated as a rule that those stratified rocks which are highly jointed, fissured and porous, furnish many easy channels for the passage of rain-water to strata situated at great depths, and that beds of clay, clay slates, fine-grained sandstones, quartzites and compact mica schists make very good confining beds. The quantity of water furnished by deep wells near Philadelphia is enormous. The well at Lansdale, Montgomery County, can yield 288,000 gallons per day; the two wells at Jenkintown, 216,000 gallons each per day; the well of the Oriental Bath Company, 1104 Walnut Street, 144,000 gallons per day. One popular error seems to have gained a foothold, that a well is of little interest unless it be a deep one, and that the deepest ones furnish the most water. Many lose sight of the fact that the object of drilling a well is to obtain water, and when an abundant supply is reached at a moderate depth it is a useless expense to drill deeper. Shallow wells often yield enormous quantities of water. A drilled water well only 35 feet deep and 6 inches in diameter, at Washington Square gives over 1,500 gallons per hour and does not lower no matter how much is pumped. (See *Proc. Amer. Philos. Soc.*, May 1, 1891.)

What kind of Rocks yield Hard Water and what kind Soft?—Whether a water be hard or soft depends mainly upon the composition of the soil and rocks through which it filters. Water that passes through calcareous or magnesium rocks of great thickness will probably be hard, while water that passes through rocks composed of silica, alumina, iron, potash or soda will probably be soft. The great deposits of limestone, marble, gypsum and other calcareous rocks, as well as the magnesian rocks, such as dolomite, chlorite and talcose schists would then yield hard water. The granites, gneisses and many sandstones and slates would furnish soft water. This, in the main, is true, although there are some exceptions which local conditions modify. Some sandstones will yield soft water, while other sandstones furnish hard water; it depends mainly upon the cement which binds the grains together; if the cementing material be carbonate of lime or sulphate of lime the water will probably be hard, especially if the well be of great depth and the water be long in contact with the rocks. If, on the other hand, the cementing material be feldspar, such as orthoclase or albite (not labradorite), or even gelatinous silica, the water will probably be soft.

In England, many determinations of the hardness of spring and artesian water from different geological formations have been made; so that it can be safely predicted what kind of water an artesian well will yield when it is drilled in the Devonian, in the Silurian limestone, in the new red sandstone, or in the chalk.

DEEP ARTESIAN WELL WATER (*England*).

	Hardness.	
Water from Devonian sandstone,	17°	Hard waters.
Water from magnesian limestone,	43°	
Water from new red sandstone,	17°	
Water from chalk,	27°	
Water from granite and gneiss, soft.		
Water from Silurian sandstones, slate and shales, soft.		
Water from millstone grit, soft.		

Water which flows through calcareous channels is hard, while that which flows through silicious rocks is soft.

The same facts are noticed in the artesian wells of Pennsylvania. Artesian water from the new red sandstone is generally hard, though not so hard as the English new red water. An analysis of a new red water from a well 102 feet deep, at Norristown, gave me 11°·8 of hardness; the hardness was mostly due to carbonate of lime, although some sulphate was present. The magnesian limestones of Pennsylvania furnish hard water; this is noticeable at Reading, Pa., where a limestone stream empties into the Schuylkill and neutralizes the water from the coal mines, which is charged with sulphate of iron and a little sulphuric acid. When the streams meet, sulphate of lime is precipitated, and also carbonate of iron, which decomposes to ferric hydrate, and can be seen as a brown stain on the dam near the city.

Many of our deep marble and limestone quarries of Montgomery County, Hitner's, Henderson's, Graver's, Rambo's and others, are always partially filled with water; this is not rain-water because at Graver's quarry the rotary force pumps were running eight months of the year night and day, with an eight-inch and four-inch discharge pipe and the quarry was not nearly pumped dry. This water filters through the limestone and is not always hard because at Rambo's quarry the water was used in the boilers and the engineer reported no scale or precipitate present. Deep artesian water from clay slates and shales of the trias is often a little hard. (See analysis of boiler scale from Lansdale artesian well water.) Deep artesian wells in the mica schist and gneiss of Philadelphia and vicinity furnish water of slight hardness; this hardness disappears when the well has been in use for several months and is not sufficient to interfere with its use for industrial purposes. The purest and clearest water in this vicinity would be obtained by drilling in the syenite and granitic rocks. The water that issues from the granitic rocks and hills above Lafayette Station is of exceptional purity and is free from organic matter.

Rate of Drilling different Rocks.—The rate of drilling through the various rocks, such as granite, syenite, mica schist, hornblende schist, sandstone, limestone, clay slates

and shales, is a very important and interesting feature as a saving of time and expense depends on it. There are several factors that enter in the problem of rapid rock drilling, the most important perhaps are the hardness and compactness of the rock, and the weight, temper and drop of the drill. The most difficult rocks to drill through are trap, quartzite, compact fine-grained sandstones, certain clay slates, granites, syenites and compact hornblende schist, obsidian, etc.

The softer rocks, such as talcose and chlorite schists, serpentine and other magnesian rocks, limestone, dolomite, hydro mica schists and many coarse-grained sandstones are readily drilled through. The following table will show the thickness of rock pierced by a chisel drill 20 feet long 5⅝ inches in diameter, weighing 700 pounds, guided so as to make a round hole:

Locality.	*Rock.*	*Rate.*
Duffield's farm, on Stony Creek, near Belfry, . . .	Clay Slate (Trias),	4½ feet drilled in 10 hours.
Ice Company's well, Norristown, ...	Sandstone (Trias),	5 " "
Kunkle's farm, Valley Green Road, near Flourtown,	Limestone (Silurian),	5½ " "
Wheadley's farm, Chester County, ...	Hydro Mica Schist,	7 " "
Wm. Janeas farm, near William Station, ...	Sandstone (Potsdam),	10 " "
Roberts well, Spring Mill, ...	Sandstone (Potsdam),	18½ " 7 hours.

The Potsdam sandstone at the above localities is not like the hard edge hill rock and conglomerate, but on the contrary is soft and micaceous, which accounts for the rapid progress. When the Schuylkill Valley Railroad was built the Pennsylvania engineers found the hardest rock they had to drill along the Schuylkill was the trap rock at Conshohocken; the next hardest was the syenite, below Spring Mill. One contractor forfeited his contract on that account; next came the mica schist, then the sandstone, and last the limestone. The minerals which compose a rock may be very hard, and yet the cementing material may hold the grains so loosely that the drill will make rapid progress through the rock. Sandstone, when composed entirely of silica, or when the cementing material is gelatinous, silica as in quartzite, is extremely hard to drill, but when the cement which binds the grains is feldspar, which decomposes readily, then the grains are loosely held, and the rock is readily drilled. The hardness of a steel drill is over 7,

while the hardness of the minerals which go to make up the great mass of rocks is under 7. Feldspar, 6; hornblende, 5 to 6; augite, 5 to 6; mica, 3; limestone, 3; dolomite, 3½; talc, 1; quartz, which has a hardness of 7, is the most abundant mineral, and rapidly wears and destroys the temper of a drill. Coarse-grained rocks are more readily drilled than fine-grained. Steeply-dipping strata are more difficult to drill on account of the trouble in keeping the drill vertical. Sayer's well, at Fox Chase, was drilled in the mica schist. There was one vein of rock in this well, probably quartz, only seven inches in thickness, that took eighteen hours to drill, and required twelve sharp drills in that time.

Cryolite in a Philadelphia Artesian Well.—Wm. W. Matos, a student of the Central High School, presented me with two specimens of minerals, which he said came from an artesian well; on examination, they proved to be cryolite. One specimen contained several rhombohedra of siderite, which frequently accompanies Greenland cryolite. The other specimen contained, besides cryolite, some siderite, galenite, quartz, and a little copper pyrites, all of which are found in the cryolite of Ivigtuk, Greenland. The specimens were about the size of common marbles. On telling him that it was unusual to find cryolite except in Greenland and a few other localities, and that there must be some error he volunteered to see the driller again and question him closely. The specimens are without doubt cryolite. The exact locality of the well is about Fiftieth Street and Columbia Avenue, on land belonging to Pennsylvania Railroad. The well was drilled by Brush & Co., a New York firm. On asking why the specimens were not ground to powder by the drill, he said that when drilling at a depth of 280 feet they struck a water crevice, and the drill fell a short distance and broke off some fragments of the surrounding rock, these were drawn up by the sand pump, and the specimens of cryolite were among them. The driller says the cryolite was broken into several small pieces, and estimates the depth at which it was broken off to be about 280 feet. He thinks the vein is only a few inches in thickness. The well was

drilled to obtain water for the locomotive engines of Pennsylvania Railroad Company. The rock passed through was mica schist and gneiss, and at and around 280 feet the driller pronounced the rock very hard. The diameter of the well is six inches, and they quit drilling at a depth of 350 feet. The water rises in the wall to within twenty-five feet of the surface of the ground, and on pumping without a force pump, and using only a small engine pump, the well yields 2,400 to 3,000 gallons per hour; it would give much more with an improved pump. No specimens were found below 280 feet. The finder was Mr. Richard Wickward.

Cost of Artesian Wells.—The price of drilling is about $2 per foot in Montgomery County for wells 6 inches in diameter and from 100 to 200 feet deep: this is independent of the character and hardness of the rock. A hard clay slate requires twice as long a time to drill as a loose mica schist or a decomposed sandstone, and yet it would be a difficult matter to regulate the price according to the hardness of the rock and the difficulty of drilling, because in many wells alternate beds of sandstone, red shale and sometimes clay are met with. An examination of the table on the rate of drilling will show that the rate with which different rocks are drilled varies greatly yet the same rate per foot is charged for granites, syenites, mica schists, sandstones and limestones. I have known the drillers to make the unusually slow progress of six inches in ten hours when a very hard quartzite was being drilled. Sometimes they are delayed more than a month over the usual time required to drill a well, by some unusual obstruction, such as a hard, steeply-dipping stratum, when it is almost impossible to keep the drill vertical and the well straight; occasionally, the tools become loosened and drop down the well, and a week or more is wasted in grappling for them. The six-inch iron pipe (internal diameter five and five-eighths inches), which is used to line the well, varies in price from forty to fifty-five cents per foot. Most of the wells in Montgomery County, which will be referred to later, and whose depth varies from 50 to 200 feet, were drilled at the rate of about $2 per foot. The driller of these wells guarantees water, and

promises to finish the well and test it or no pay. If the water should be chalybeate or colored, which is rarely the case, he agrees to pump it until it becomes clear.

In Philadelphia, the charges vary and contracts differ in terms. Some drillers contract, for a certain specified sum, to drill a well that will furnish a certain amount of good water per minute, generally fifty or seventy-five gallons. The contract price includes everything, casing, coal and testing.

Many of the Philadelphia wells are double-cased; the first pipe put down is an eight-inch pipe; then a six-inch pipe is put down inside of this, and the space between the two is filled with hydraulic cement.

Some contracts have been made at the rate of $4 per foot, this includes the drilling, casing, testing and everything connected with the well.

Other contracts have been made at the rate of $2.75 per foot for drilling down to 500 feet and $3 per foot for drilling below a depth of 500 feet; this does not include the iron pipe for casing, but only the drilling.

Deep Artesian Wells in the Red Shales and Clay Slates of the Triassic.—At Lansdale, Montgomery County, Pa., on the line of the North Penn Railroad, three artesian wells were drilled for Effrig & Son, pork packers.

The first well is 159 feet deep, the second one 376 feet, and the third and latest one is 611 feet deep. These wells are within fifty feet of one another. The last well has 259 feet of iron pipe as casing, and, on pumping with a small pump, gives seventy gallons per minute; with a larger and improved pump, it will give 200 gallons per minute, or 288,000 gallons per day: the supply seems to be practically inexhaustible. The water rises to within 140 feet of the top of the well.

No systematic record of the borings was kept. Rock was first struck at a depth of twelve feet.

The drill passed through alternate layers of red shale, red sandstone, blue clay slates, and black carbonaceous slates. A small bed of carbonaceous slate, six or eight inches thick, was struck, and also a bed of the same character, twenty

feet thick. This last bed was found at a depth of forty feet from the surface. Under this bed of black clay slate was a deposit of what they supposed to be cannel coal, but it was evidently a shale or slate, very rich in carbon.

The water from these wells when used in boilers deposits a scale, which I analyzed and found to consist of $CaCO_3$, $MgCO_3$, $CaSO_4$, $MgSO_4$, NaCl, and a trace of iron, sufficient to color slightly the scale. The water is not very hard, the hardness being mainly due to $CaCO_3$, which makes up the bulk of the scale. This hardness of the water is only temporary and will disappear when the well has been in use for several months and continually pumped, as fresh water takes the place of that which has long been in contact with rocks and which has dissolved out the salts of lime and magnesia. This I have noticed to be the case in several wells; when the well is first drilled, the water is hard enough to require a small amount of soda in the boiler as an anti-incrustator. It acts by precipitating the lime and softening the water. After the well has been pumped for several months the hardness disappears and the anti-incrustator is dispensed with.

The 611-foot well was six months in drilling. The drillers complained that the clay slate was very hard and contained much quartz, which kept them drilling much longer than usual.

The above facts correspond well with those obtained from another well drilled a few miles from Lansdale on, the Duffield farm, between Custer and Belfry, on the Stony Creek Railroad. (See *Proc. Amer. Philos. Soc.*, vol. xxix, May 25, 1891.) In this last well, the bed of very black rock, supposed to be coal, was found at a depth of about forty feet from the surface; this is the same depth as the Lansdale bed. I understand the same bed of coaly rock was found when Mr. Rozenzi drilled the very deep well in Bucks County for gas. Another firm drilled the Duffield well, and they made the same complaint about the hardness of the rock. One bed in particular, that was supposed to be quartzite, was so hard that only six inches of it could be drilled in ten hours. The

Duffield well is only sixty-five feet deep and yields but sixty gallons per hour, while the Lansdale well yields 200 gallons per minute. In these tightly-packed clay slates an abundance of water is found only at great depths.

BOOK NOTICES.

William Gilbert, of Colchester, Physician of London. On the loadstone and magnetic bodies, and on the great magnet, the earth. A new physiology demonstrated with many arguments and experiments. A translation by P. Fleury Mottelay. New York: John Wiley & Sons, 53 East Tenth Street. 1893.

The publishers are to be congratulated on the appearance, in admirable form, of a translation of the classic work of Gilbert, at the hands of one so competent, by reason of his special familiarity with the subject, as Mr. Mottelay. The task which the translator had to perform was one which required the exercise of the severest critical acumen to render into modern idiomatic English the often obscure meaning of the author, and a thorough familiarty with the modern aspects of electrical theory and practice. That the author has done his work well, is demonstrated by the extremely flattering reception with which it has been received by specialists everywhere. W.

The Mineral Industry. Its statistics, technology and trade, in the United States and other countries, from the earliest times to the end of 1892. Vol. I. Edited by Richard P. Rothwell, editor of the *Engineering and Mining Journal.* New York: The Scientific Publishing Company. 1893.

For a number of years the *Engineering and Mining Journal* has engaged in the collection of statistics relating to the leading features of the mineral industry, which were published annually in the form of a statistical supplement. These supplements for a series of years constituted a valuable source of information to those interested in these important industries, from which to obtain reliable advices respecting their condition; and, from the thoroughness with which the work was done, these supplements came to be regarded as standard publications, whose appearance was eagerly looked for. They were all the more welcome from the fact that they constituted the only source of early information accessible to those engaged or interested in the great mineral producing industries. The annual volumes issued under the direction of the Statistical Bureau of the United States Geological Survey, have, by force of circumstances, invariably been so long delayed, that when they appeared their contents, though of great value for purposes of comparison and future reference, were of comparatively little importance for immediate use to business or professional men.

Encouraged doubtless by the very flattering reception accorded to the statistical supplements, the *Journal* conceived the ambitious project of extending their scope, and, beginning with the year 1892, has begun the publication of an annual volume in book form, comprising a complete summary of the

statistical position, technology and trade of the mineral industries of the United States and other countries. The first volume of this interesting and valuable publication is before us, forming a substantial quarto of over 600 pages.

Each of the important subjects embraced in the volume is treated in a separate chapter, under the signature of a competent specialist, and the entire collection of contributors has had the advantage of the editorial revision of Mr. Rothwell, whose skill and long experience in work of this kind need no praise at our hands.

Only those who have been engaged in statistical work will be able to appreciate fully the amount of labor involved in the work of collecting and revising the vast array of facts and figures contained in a volume of this character, and the additional labor involved in its preparation and publication within a few months of the close of the year to which they relate, a circumstance which greatly enhances its value to the professional and business community. The volume is a surprising and gratifying evidence of journalistic enterprise on the part of its publishers, and reflects the highest credit upon all who have contributed to its make-up.

We are assured that it is the purpose of the publishers to issue this volume annually. It is by far the most complete and valuable publication of its class that has ever been issued, and if the declared intention of its publishers is carried out, it will constitute a work of reference which will be absolutely indispensable to all who are interested in the subjects of which it treats. W.

Knots, Splices, Hitches, Bends and Lashings. Illustrated and described. By F. R. Brainard, Ensign, U. S. N. New York: Practical Publishing Company, 21 Park Row. 1893. (Price, $1.)

This little manual explains and illustrates very satisfactorily the various forms of knots, splices, lashings, etc., which comprise so important a portion of the mystery of the art of the seaman, canoeist, hunter, rigger, builder and mechanic, and can be usefully consulted by all who wish to inform themselves of the subjects of which it treats. In addition to the descriptive and illustrative portions, the work contains a useful set of tables of hemp, and iron- and steel-wire rope, and a dozen pages of definitions of technical terms peculiar to the several arts and crafts above-named. W.

Valve Gears for Steam Engines. By Cecil H. Peabody. New York: John Wiley & Sons. 1893.

The present work employs both the analytic and graphic methods for studying the distribution of steam, using the Zeuner diagram.

After an analysis of the effect of the plain slide valve, the effect of shifting eccentrics, link-motions, radial and double valve gears are examined, special attention being paid to the methods of compensating for the inequalities introduced by the oscillation of the connecting-rod. The last chapter is devoted to a study of those valve gears which effect the cut-off by a disengaging of the valves from the valve-moving mechanism, permitting dropping weights to close the valves. H. B.